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PROFESSOR YOUNG ON THE SUN.

PROFESSOR YOUNG, although no one would imagine it from the book before us, is one of those to whom we owe some of the most important of the discoveries which, during the last few years, have added so much to our knowledge of the sun. It is well that we should have treatises such as this from the workers themselves to whom our knowledge is due. For, though the most skilful observers are not always the ablest either in dealing with known facts and deducing sound theories from them, or in presenting them to the unscientific world, there is always in their writings a special value and something of the charm which we find in accounts of travel by those who have seen what they describe. Professor Young, however, possesses much more than mere observing skill. He is a sound and careful reasoner, and if there is a certain terseness and preciseness (more than mere precision) in his writing, which detracts a little from its literary charm, this has probably been rendered necessary by the limited nature of the space at his disposal, and in no sense deprives his work of its claim to be regarded as exceedingly well written. This treatise possesses also another quality, very important, we conceive, in astronomical writing.* Professor Young is a mathematician, and the formulas he gives, even when they are not of his own devising, are given with adequate understanding of their meaning and value, which, unfortunately, has not been the case with all the treatises on astronomy recently published.

Taking first Professor Young's treatment of the dimensions, mass, and power of the sun, we note that he adopts for the sun's distance an estimate very near that which the labours of his countryman, Professor Newcomb, seem to indicate as nearest to correctness, viz.: 92,885,000 miles, with a probable error of about a quarter per cent., or 225,000 miles. This would correspond with a mean equatorial horizontal solar parallax of 8″·80, or, in untechnical terms, the earth seen from the sun at his mean distance, would have an apparent maximum diameter of 17″·60 (about one 106th part of the apparent mean diameter of the moon or of the sun as we see these orbs from the earth.)* We may note in passing, that in comparing the methods of determining the sun's distance by observations of Mars on the stellar heavens and of Venus in transit, Professor Young omits to notice that whereas the full displacement of Mars, as seen from different parts of the earth, is available, the displacement of Venus on the sun's disc is only a part of her actual displacement, the sun himself being displaced (roughly, only about seven-tenths of the displacement of Venus is available). His account of the American photographic method of observing the transit of Venus is full of interest, though calculated to make Englishmen somewhat ashamed of the relatively unscientific method which (despite good advice to the contrary) was adopted at the English stations.

The mass of the sun, deduced from this estimate of the distance, is about 330,000 times that of the earth. Expressing this in tons, Professor Young uses what we take to be the erroneous American system of notation, saying that the earth's mass amounts to about two octillions of tons, where we should say two thousand trillions of tons. Respecting the sun's attractive power, as exerted on the earth, Professor Young quotes the following impressive illustration from a recent calculation by Mr. Warring:—"We may imagine gravitation to cease, and to be replaced by a material bond of some sort, holding the earth to the sun, and keeping her in her orbit. If, now, we suppose this connection to consist of a web of steel wires, each as large as the heaviest telegraph wires used (No. 4), then, to replace the sun's attraction, these wires would have to cover the whole sunward hemisphere of our globe about as thickly as blades of grass upon a lawn. It would require nine to each square inch."

It should be added, however, and still further enhances our conceptions of the sun's might, that were the connection between the sun and earth of this nature—that is, by steel wires—more than three hundred days would be required to make the pull of the sun felt at the earth. The action of gravity is exerted certainly in less than a second. In fact, the most careful observation of the planet's motions reveals no evidence that gravity takes even any appreciable time at all in traversing the spaces separating the various members of the solar system from each other. This apparent instantaneity of the action of gravity is one of the greatest mysteries known to science.

Sir John Herschel has well remarked that Giant Size and Giant Strength are little without Giant Benevolence. It is the light poured forth by the sun on the planets, the heat whereby he nourishes them, more than his vast bulk and his mighty mass which fit him to be the central ruler

"The Sun." By C. A. YOUNG, Professor of Astronomy in the College of New Jersey. (Messrs. Appleton & Co., New York.)

* "To borrow the curious illustration of Professor Mendenhall," says Professor Young, "if we could imagine an infant with an arm long enough to enable him to touch the sun and burn himself, he would die of old age before the pain could reach him, since, according to the experiments of Helmholtz and others, a nervous shock is communicated only at the rate of about 100 feet per second, or 1,637 miles a day, and would need more than 150 years to make the journey. Sound would do it in about 14 years if it could be transmitted through celestial space, and a cannon-ball in about 9, if it were to move uniformly with the same speed as when it left the muzzle of the gun." These illustrations are striking—we have seldom used them in lecturing on the sun without noticing that they produce a strong impression on the audience. But we find another illustration in the book before us, still more impressive. While the earth is travelling 20 miles along her circular path, her path deviates from the tangent at the first point of the arc of 20 miles by only one-eighth of an inch. This is the distance which the sun, with all his tremendous attractive energy, draws the earth towards him in a second of time.

XUM

of a scheme of circling worlds. It is estimated that the intensity of sunlight at the sun's surface exceeds 19,000 times that of a candle flame; 5,300 times that of metal in a Bessemer converter; 146 times that of the calcium light; 3·4 times that of the electric arc at its greatest attainable brilliancy. The heat emitted by the sun in every second of time is as much as would be obtained by the consumption of 16,436 billions (millions of millions) of tons of coal per second.

Passing to the consideration of the aspect of the mighty globe, which thus rules, lights, and nourishes the earth and her fellow planets, we find in this treatise one of the most complete, though concise, accounts of telescopic study of the sun which has yet been published. The nature and appearance of the spots, the faculæ and the granules, the way in which the spots are formed, vary in structure and disappear, their motions in different solar latitudes, and other phenomena of interest, are described succinctly and reasoned upon with skill and caution. We must leave, however, our remarks on this important part of Professor Young's subject to another occasion.

PRECESSION OF THE EQUINOXES.

By the Editor.

SINCE the whole globe of the earth reels in the way described in No. 11, while the sphere of the fixed stars remains all the while unchanged in position, the poles of the heavens (the points towards which the axis of the earth is directed) must move in circles around the poles of the ecliptic (the points towards which a perpendicular to the plane of the earth's motion is directed). The inclination of the earth's axis being about $23\frac{1}{2}°$, the circles thus described by the poles have an arc diameter of about 47°. In our illustrative map we show a part of the circle described by the north pole of the heavens around the pole of the ecliptic. This part shows the course pursued by the pole from between five and six thousand years B.C. to about 8500 A.D.

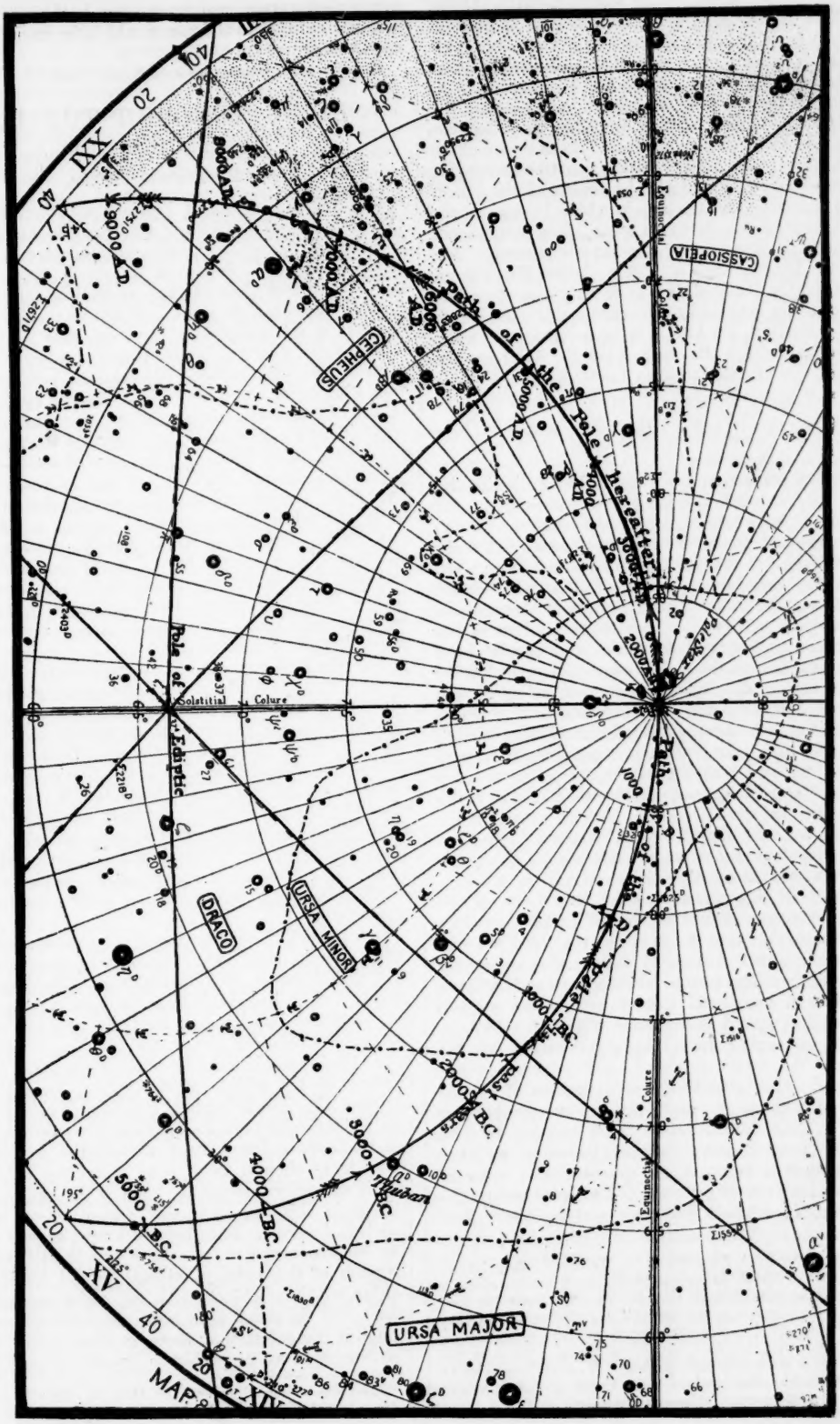
We must, however, note two circumstances in the part of the North Pole's path thus given. There is a reeling motion due to what is called nutation, a sort of small reel executed in about eighteen and a half years, the effect of which is to make the course of the pole wavy, instead of the simple circular path we have shown; this does not much affect the accuracy of the picture. There is another and somewhat more important correction, which would not, indeed, make much difference in our map, but in a map of the pole's course, on a larger scale, would have to be taken into account. The pole of the ecliptic is itself moving. The plane of the earth's path is slightly inclined to the median plane—the so-called invariable plane—of the solar system. This invariable plane is nearly coincident with the plane of Jupiter's orbit, but not quite. It crosses the ecliptic ascendingly, in about longitude 103°, and is inclined to that plane at an angle of about 1° $35\frac{1}{2}'$ (the plane of Jupiter's orbit crosses the ecliptic ascendingly in longitude 99°, and is inclined to it at an angle of about 1° $18\frac{3}{8}'$). The pole of the ecliptic describes then a small circle about the pole of the invariable plane, this small circle having a diameter of about 3° 11′. The time of circuit is not as yet known, because of uncertainties which exist as to the exact masses of the members of the solar system. Suffice it here to notice that, owing to this change, it is calculated that, during the last 3,000 years, the pole of the ecliptic has diminished its distance from the pole of the equator by about 25′. That the physical theory is not far from correctness is shown by the circumstance that, from observations made by Tcheou King 1,100 years before the Christian era, the obliquity would seem to have been then about 26′ greater than at present.*

Apart from this slight change, the path shown for the pole in our illustrative map is sufficiently near to correctness. We note that the only conspicuous star which has been very near the pole during the last five or six thousand years is the star Alpha Draconis, or Thuban, yet this star must have been far more conspicuous in Bayer's time (still more, therefore, in all probability, in the time when it was the Pole Star) than it is in our own; for Bayer lettered the stars in each constellation in the order of their brightness, as nearly as he could estimate that order with the comparatively rough light-measuring methods available in his time; and the most cursory study of the stars of the Dragon shows that both Beta and Gamma are now much brighter than Alpha.

Thuban was nearest the pole about 2,700 years ago; but, of course, it was the Pole Star for a long time before and after the period when it was nearest the pole (just as Alpha of the Little Bear is now the Pole Star, though some 300 years will elapse before it is at its nearest to the pole). In dealing with the Great Pyramid, and trying to determine when it was built, it is not the time when Thuban was nearest the pole that we have to consider, but the time when it was at a certain definite distance (about 3° 40′) from the pole. Now, a study of the pole curve in our map shows that the star Thuban was at this distance from the pole (about $\frac{1}{15}$ths of the distance between the successive circular lines in the map) at two epochs. We open out a pair of compasses to the distance just named, and placing one point on α Draconis, describe with the other a circle; this will cut the path of the pole in two points, one corresponding to about 3350 B.C., the other to about 2170 B.C. Either of these would correspond with the position of the descending passage in the Great Pyramid; but Egyptologists tell us there can absolutely be no doubt that the later epoch is far too late. If, then, we regard the slant passage as intended to bear on the Pole Star at its sub-polar passage, we get the date of the Pyramid assigned as about 3350 years B.C., with a probable limit of error of not more than 200 years either way.

Be this as it may, we know that in the past the constellation of the Dragon was at the pole or boss of the celestial sphere. In stellar temples, like those of which Rawlinson gives examples, the Dragon would be the uppermost or ruling constellation. And here, in passing, it may interest the reader to note that some find evidence in this relation, that when writers of old spoke of the Old Dragon as having been cast from heaven, carrying two-thirds of the celestial

* Fellows of the Astronomical Society will remember, with some amusement, how, a few years ago, Lieut.-Col. Drayson (a mathematician of some skill) gravely told astronomers of the observed diminution of the ecliptic obliquity, as if it were some unexplained change, instead of being one of the most beautiful confirmations of the theory of gravity. On the one hand are the calculated effects of the perturbing action of the several planets; on the other, an observed change, precisely corresponding with calculation; yet, instead of "confirmation strong" of accepted views, Col. Drayson could only find a source of perplexity. On this imaginary difficulty he based an entirely impossible theory of the glacial period. Yet, when he described his views about the changing obliquity of the ecliptic, one of the greatest mathematicians living explained to him that the change he found so perplexing was a necessary consequence of the action of gravity. He had been too many years fostering his delusions to give them up. Such is the birth and growth of paradoxes: an error which could have been readily corrected at the outset, sets a man wasting years of labour in developing a false theory; and when, at last, his error is corrected, he is too much in love with his own work to give the error up.


CASSIOPEIA
CEPHEUS
DRACO
URSA MINOR
URSA MAJOR
Path of the Pole hereafter
Pole of Ecliptic
Solstitial Colure
Equinoctial Colure
Pole Star
Thuban
Past Pole
Path of Pole
5000 A.D.
6000 A.D.
7000 A.D.
9000 A.D.
2000 A.D.
3000 A.D.
1000 A.D.
2000 B.C.
3000 B.C.
4000 B.C.
5000 B.C.
MAP

beings with him, reference was made (unconsciously, perhaps, on the narrator's part) to some tradition of the passing away or fall of the Dragon from its former ruling position among the constellations. Those who thus interpret ancient records (much more ancient than Jewish history) find in Hercules, with his heel assailed by the serpent, as in our constellation figures, the first Adam; in Ophiuchus, the serpent bolder, the second Adam. In Argo they find the Ark—in fact, in a whole series of constellations they find the story of the Flood. In Aquarius, with the streams pouring from his water-jug, they find the beginning of the Flood. In the river Eridanus and the seas in which Pisces and the great sea monster, Cetus, seem to swim, they see pictured the prevalence of deep water over the whole earth. The Raven of the heavens is the Raven of the Flood-narrative. Argo is the Ark, shown as if only the stern half of a great ship lodged in the mountain. The Centaur, bearing sacrifice, as Aratus says, to Ara, the altar, is Noah offering sacrifice after he had left the Ark; and the Bow of Sagittarius in the smoke (the Milky Way) which seems to ascend from the altar,—

Ara ferens thuris, stellis imitantibus, ignem,—

is the Bow of the Promise.

These may, of course, only be fancies, but it is singular how closely these constellations, which are among the few really seeming to picture recognisable objects in the heavens, correspond in sequence and in range of right ascension with the events recorded respecting the Flood. For my own part, I am not of those who wonder that the ancients should have seen figures of the animals and other objects with which they were familiar in the heavens. From my boyhood upwards (and the boyhood of the individual is like the childhood of the race) I have seen figures among the stars, the figures being always such as I was familiar with. Even so late as my recent visit to the southern hemisphere, I found that almost despite myself the novel-looking star groups formed figures* with which I was ever after compelled to associate them; and I cannot doubt that it was the same with the childhood of the human race. There is certainly a well-shaped ship where Argo was seen by the ancients; the water streams of Aquarius and Eridanus are distinctly visible; Ara is a well-shaped altar; and though at present the figure of the Centaur (the man part) is not so upright as it was before precession had tilted it over (as it has the ship), we can still see there the figure of a portly man bearing something towards the altar. The bow is clearly seen, and nothing can be better in accidental picturing than the curling streams of smoke (figured in the stars of the Milky Way), which seem to ascend from the flat summit of the altar.

But precession has altered the configuration of all the star-groups as seen when most favourably situated for observation. Take a star globe, and, holding it with one forefinger near Thuban (Alpha Draconis), and the other at the opposite point of the sphere, notice the constellations as they slowly rotate. Note how steadily the ship sails past its highest point, on upright keel; how upright the Centaur and the Altar;* and how many groups, now almost unrecognisable, are seen in their new aspect to be fairly entitled to the names which the ancients bestowed upon them.

* I had always expected, from Sir John Herschel's description, to find Orion when inverted, as we see him in the southern skies, a very noble and impressive figure. But the very first time I so saw it, I immediately recognised in the figure, Clive Newcome's picture of Fred Bayham, and I never afterwards saw the constellation without at once seeing in it that ridiculous figure. When people tell me they cannot see a Bear in Ursa Major, I can only wonder at their blindness; the head of the bear being to me as obvious and as obviously ursine as a group of stars could well be.

SEEING THROUGH THE HAND.

AN OPTICAL ILLUSION.

By Thomas Foster.

THERE is a series of illusions affecting the apparent shapes and positions of solid figures—not only regular geometrical figures, as prisms, parallelopipeds, &c., but all solids whatever — drawn as if formed of a transparent material, so that their farther as well as their nearer outlines or edges can be seen. It can be readily shown that there is a law connecting in every case the false figure with the real figure. I have prepared a paper on illusions of this sort, with suitable illustrations, and another paper with illustrations on some curious cases of apparent motion in sets of concentric circles. But these and other papers on illusions are, it appears, kept over for the present by the pressure of other matter. [Circles next week.—Ed.]

In the meantime, I wish to submit to readers of Knowledge (as occupying less space) an illusion which seems to me exceedingly instructive, as bearing on the question how we see. Everyone knows that the eye itself is simply the organ by which the optical nerve is affected by light, and that it is by this nerve that the brain becomes cognisant of these light effects, the brain interpreting the messages brought along the optical nerve into information respecting the objects of sight.

Now the two eyes, and the optical nerves which extend to each, convey at all times different messages to the brain, which yet, as a rule, combines the two sets of messages into a consistent account (so to speak) of what is seen with both eyes. Even when the eyes differ in focal length, so that, as separately analysed, the views obtained by the two eyes are utterly unlike, the mind is very seldom perplexed by the two different accounts conveyed to it. But in the following experiment the eyes entirely deceive the mind, conveying to it the absurd impression that there is a hole right through the palm of the hand, or of a book or other opaque object which may replace the hand in the experiment.

Roll a sheet of card or paper (or the number of Knowledge now in your hand) into a tube nine or ten inches long, and about an inch in diameter. Holding this tube with the right hand, say, look through it with the right eye, while the left hand is held six or seven inches from the eye, the palm facing you, and touching the tube a little below the lowest joint of the little finger, that is, at about the level of the middle of the palm. Then, if both eyes are open, the tube being held touching or close to the right eye, while the left eye looks at the left palm (at about the nearest distance for distinct vision), the appearance presented is as though there were a circular hole about an inch in diameter through the palm of the left hand.

Now, in this case, the mind does not need to be told that it is deceived. The observer knows as well as possible that while he seems to be looking with the left eye through the palm of the hand at objects beyond, he is in reality looking at those objects with the right eye through the tube. Yet the mind does not correct the illusion, clearly though it recognises that there is illusion and its nature. The illusion is as preposterous as that experienced when

* Only the modern figure of the Altar is absurdly drawn upside down. In old globes and charts we find it properly drawn.

crossing the second finger over the first, we feel the tip of the nose with these fingers thus interchanged in relative position. In this case the observer seems to feel two nose tips, though he knows certainly that he has but one; he knows, too, just how the illusion is occasioned, but for all that the illusion remains.

The two most trustworthy senses, sight and feeling, being thus able to deceive—to palm off upon us, so to speak, what each of us knows to be false,—we see (and feel) how very far from the truth is the saying that seeing or feeling is believing. If these senses deceive us when we know precisely what they are doing (as when conjurors explain and illustrate in action the manner of their tricks), is it to be supposed that they do not often deceive us, and still more thoroughly when we have no means of testing what they tell us by what we already know?

INTELLIGENCE IN ANIMALS.

IN the case last considered, we see that a dog, belonging to a species not distinguished for keenness of scent, was not long deceived by a picture, even under circumstances favouring the deception—as his previous sleep, the position of rest from which he saw the figure, and the strong light shining upon it. As this was the only instance known to one who was familiar with the ways of dogs, the negative evidence respecting the recognition of pictures by animals is rather strong. However, there have been cases where animals, if not actually deceived by a picture, seem certainly to have understood what it was intended to represent. The following case seems to me full of interest. It is related by Mr. Chas. W. Peach, of Edinburgh. He remarks, first, that in certain publications dogs are said never to have recognised a painted likeness. "During my residence in Cornwall," he goes on to say, "I had a most intelligent and faithful dog for fifteen years. I had him when a month old. His mother was a beautiful liver-coloured spaniel, rather large; his father, a black Newfoundland; my dog took after him in colour and shape. In 1843, a young and self-taught artist asked me to allow him to paint my likeness in oil colours, and I consented. His studio was in the next town, three miles distant, and, as often as required, I went over. I, however, did not take my dog with me. It was done in "kitcat" size, and he succeeded so well in the likeness and artistic work, that, when exhibited at the annual meeting of the Polytechnic Society at Falmouth, a medal was awarded for it; and, as well, it was highly commended. The artist was so grateful that he presented me with the painting, and I still have it. When it was brought to my house, my old dog was present with the family at the "unveiling"; nothing was said to him nor invitation given him to notice it. We saw that his gaze was steadily fixed on it, and he soon became excited, and whined, and tried to lick and scratch it, and was so much taken up with it, that we, although so well knowing his intelligence—were all quite surprised; in fact, could scarcely believe that he should know it was my likeness. We, however, had sufficient proof after it was hung up in our parlour. The room was rather low, and under the picture stood a chair; the door was left open without any thought about the dog; he, however, soon found it out, when a low whining and scratching was heard by the family, and on search being made, he was [found to be] in the chair trying to get at the picture. After this I put it up higher, so as to prevent its being injured by him. This did not prevent him from paying attention to it, for whenever I was away from home, whether for a long or short time—sometimes for several days—he spent most of his time gazing on it, and as it appeared to give him comfort, the door was always left open for him. When I was long away he made a low whining, as if to draw attention to it. This lasted for years, in fact so long as he lived and was able to see it. I have never kept a dog since he died; I dare not, his loss so much affected me."

A similar anecdote is related of a painting by the elder Phillips. "Many years ago," says the lady who narrates the tale, "my husband had his portrait taken by T. Phillips, sen., R.A., and subsequently went to India, leaving the portrait in London to be finished and framed. When it was sent home, about two years after it was taken, it was placed on the floor against the sofa, preparatory to being hung on the wall. We had then a very handsome, large, black-and-tan setter, which was a great pet in the house. As soon as the dog came into the room, he recognised his master, though he had not seen him for two years, and went up to the picture and licked the face. When this anecdote was told to Phillips, he said it was the highest compliment that had ever been paid to him."

We have seen how a bull-dog, the least intelligent, perhaps, of all dogs, behaved in presence of a portrait. We have now to consider the behaviour, under similar circumstances, of the British mastiff, a more intelligent animal than the bull-dog, though not regarded as standing by any means first among dogs in this respect. The particular mastiff in question is one to whom I had the pleasure of being introduced some five years since, Dr. Huggins' dog Kepler. He is worthy of a brief biographical sketch. He was a son (that is, Kepler was) of the celebrated Turk, and was born about the year 1871. "He stands," wrote Mrs. Huggins of him, towards the close of 1876, a few months before his lamented decease, "thirty inches high, and is lion colour on the body; his face, the tips of his ears, and the tip of his tail, are marked with black. In disposition he is usually exceedingly affectionate and gentle, though he can be otherwise. Probably he thinks [though here I must confess that, strongly though Mrs. Huggins's opinion would support my case, I cannot altogether agree with her] that the words of George Herbert may apply to dogs as well as to men, and so reasons that—

> He is a fool who cannot be angry,
> But he is a wise *dog* who will not.

He has a clear idea of his duty in life. As Mr. Carlyle would say, 'he has found his work to do,' and considers it to be—to borrow the expression of an old writer—'to kepe his mastre and his maistirs hous.' To this end he is continually on the watch, barking in quite different ways as different comers approach. He has a bark of welcome for those he loves; of courtesy for mere acquaintances; of inquiry for strangers; of warning against enemies," an approach here, one may say, to language.

Kepler first attracted scientific attention by a peculiarity which, most probably, must be regarded as a result of instinct, or as, at any rate, inherited, since nothing in Kepler's own life explains it as the result of any process of reasoning. "When he," that is Kepler, "was very young," writes Mrs. Huggins, "his master discovered on taking him for a walk one day, that he" (Kepler, not Dr. Huggins) "was very much frightened at the sight of a butcher's shop, and some little time afterwards, when he was out with a servant, the feeling again showed itself, but in a much more marked manner. On this occasion Kepler threw himself upon the ground near the butcher's, exhibiting every appearance of terror, and as no amount of coaxing could induce him to pass the shop, the servant was at last obliged to bring him home again. His master, upon this, wrote to Mr. Nicholls, from whom he had purchased

Kepler, asking if he could throw any light upon this strange dislike. Mr. Nicholls replied that it had been strongly marked in Kepler's father and grandfather, and was unusually strong in one of his brothers, so much so, indeed, that he would fly at a butcher, even when dressed in plain clothes. These facts being very striking, Mr. Huggins—Kepler's master—wrote details of them to Mr. Darwin, who was so much interested, considering the circumstances a clear instance of inherited antipathy, that he sent an account of them to *Nature*. The facts attracted much attention at the time, and various theories were put forward to explain them. In connection with this dislike to butchers shown by Kepler and his relations, it is interesting to notice that a similar antipathy is noted by Jesse to dog-killers, as mentioned both by Lord Bacon ('Sylva Sylvarum'), and Sir Kenelm Digby ('Treatise on the Nature of Bodies'), as having been common among dogs in their times. The passage from Sir Kenelm Digby's Treatise runs thus: 'We daily see that dogs will have an aversion from glovers, that make their ware of dogs' skin; they will bark at and be churlish to them, and not endure to come near them, though they never saw them before.' Dog-killing was an old custom in August." Perhaps, after all this intense aversion to butchers, dog-killers, and others who may be supposed to bear about them some scent of blood, suggesting to the dog-mind the slaughter of his kindred, may be an effect of reasoning, not, as I have suggested above, of instinct only. A dog may argue that the scent can only be explained in one way, and that the explanation is such as to suggest danger to himself—"*hinc illæ lachrymæ*."

EXCAVATIONS AT THE PYRAMIDS.

(From *The Academy*.)

The Tombs, Gheezeh Pyramids. *Nov.* 26, 1881.

DURING the past six weeks excavations have been carried on by me here, under the authorisation of M. Maspero, not for obtaining portable antiquities, but for deciding questions of architecture and measurement. Many points of interest have been uncovered for the first time in modern history, though the work was not on a large scale, and the number of excavators never exceeded twenty. There have been over 280 holes sunk, varying from a foot deep to shafts twenty feet deep and trenches ninety feet long.

A brief notice of the work done may be worth giving at once, without waiting for the complete publication of it, along with my survey of the pyramids (made during five months of last season), to which it is a necessary sequel, for fixing the exact fiducial points of the ancient constructions.

At the Great Pyramid, the entrance passage has been cleared enough to examine it throughout, and to enter the subterranean chamber freely. Some of the loose gravel in the "grotto" of the well has been moved, showing that there is a natural vertical fissure filled with the gravel. The casing and pavement of the pyramid have been found *in situ*, at about the middle of the west, east, and south sides; it was already exposed on the north side, on which alone it has been hitherto known. The outer edge of the rock-cut bed of the pavement has been cleared in parts of the sides, and at the north-east and south-west corners. The great basalt pavement has been cleared in parts, and the edge of the rock-cut bed of it has been traced along the north-east and south sides; but its junction with the limestone pyramid paving (which is at the same level) could not be found, as both are destroyed at that part. The ends of the great trenches around the basalt pavement have been partly cleared. The bottom and sides of the east-north-east trench have been cleared in parts to show the form. No bottom was found under nine feet of sand in the north trench. The small north-north-east trench has been cleared in parts up to its inner end at the basalt paving, where it is much smaller, and forks into two. The various rock cuttings and trenches north-east of the pyramid have been cleared and surveyed, but refilled, as the road passes over them. A piece of the casing of the pyramid, found near the base on the west side, has Greek inscriptions, apparently Pto .... Sōt .... (perhaps Ptolemy VIII., as the s is round); and Markos K ...., over which is hammered roughly ... m a j ... in Arabic. Nothing, besides a few fragments with single letters, had been previously discovered of the many inscriptions that existed on the casing.

At the second pyramid the corners have been all cleaned. The site of the edge of the casing has been found in six places near the corners, and the casing itself uncovered at the south-west. The edge of the bed of the pavement has been found on the north and west sides. The peribolus walls of the pyramid have been cleaned in many parts, showing that they are all carefully built, and not of "heaped stone rubbish," as had been hitherto supposed. Also, the so-called "lines of stone rubbish" on the west side of the pyramid prove to be all built walls, forming a series of long galleries about sixty in number, each about 100 ft. long, 9 ft. wide, and 7 ft. high, with ends and thresholds of hewn limestone. They would suffice to house two or three thousand men, and I can only suppose that they were the workmen's barracks. Fragments of fine statues in diorite and alabaster were found here, like those in the temple of this pyramid. The great bank of chips on the south side of the cyclopean wall north of the pyramid proves to have retaining walls built in it to hold up the stuff. The peribolus wall on the south-south-east of the pyramid is of fine limestone, of good workmanship, like most of the tombs of the period. The enormous heaps of rubbish south of this wall were slightly cut, and found to consist of tipped out, stratified, clean chips of limestone, like the rubbish banks of the Great Pyramid, but inferior stone.

At the third pyramid, the granite casing has been uncovered at its base in five places near the corners. The peribolus walls have been cleared in many parts all round, and found, in every case, not to consist of heaped stones, but to have carefully-built vertical faces, like the second pyramid peribolus, but of inferior work; and the wall on the south side is better built, and very wide.

The small pyramids have not been cleared for lack of time, as they are rather deeply buried; but a part of the rock-cut bed of pavement of the northern one near the Great Pyramid was accidentally uncovered close to the edge of the bed of the basalt pavement.

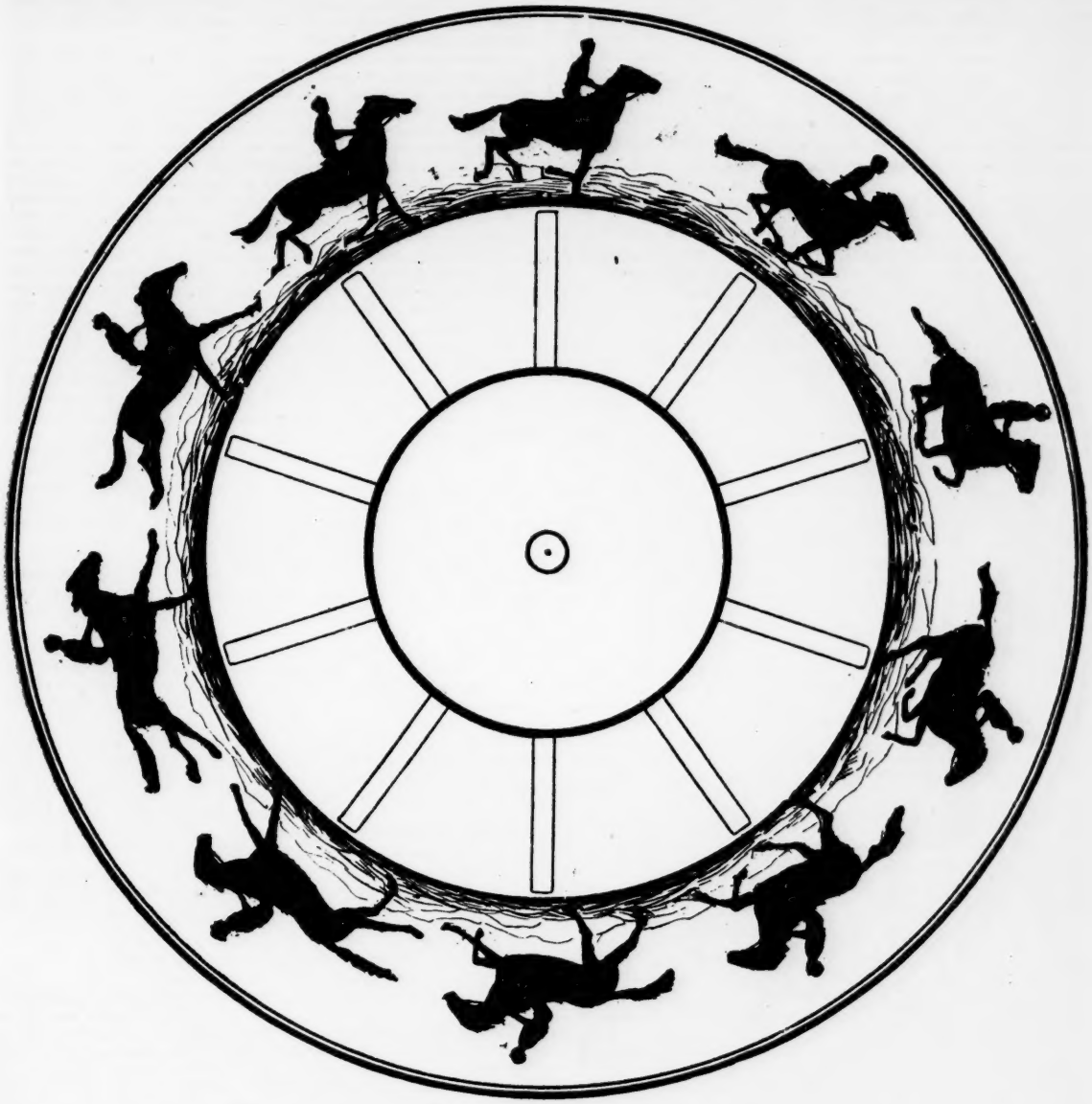
Though I am obliged to suspend work here at present, yet I shall be very glad to receive any suggestions of points needing examination (addressed to Poste Restante, Cairo); and, if they are practicable, I may find an opportunity for further work two or three months hence.

When all the paper work of this survey is finished, we shall know the sizes and distances of the pyramids within a quarter of an inch; and there will be fresh soil for the growth of theories, *as the Great Pyramid proves to be several feet smaller than hitherto supposed*, the sockets not defining the casing at the pavement level, though defining it, perhaps, at their own respective levels.

W. M. Flinders Petrie.

[Let not pyramidalists despair. It will be just as easy to find all the features of the solar system in the Pyramid with the new measures, as it was before. The new coincidences will be worth just as much, too, as the old ones.—Ed.]

Manufacture of Gas from Wood.—When wood is burned, the chief product of the combustion is a gas called carbon dioxide or carbonic anhydryde (CO_2), *i.e.*, one of carbon united with two of oxygen. The gas is the same which gives the effervescence to soda-water, &c. This fact is made use of in New York, by a process called Wilkinson's, for the manufacture of wood-gas. CO_2 is itself a non-supporter of combustion, extinguishing a light when plunged into a jar of this gas, when it is inhaled destroys life, as in the fatal valley of Java (Johnston's "Chemistry of Common Life"), or anywhere else where persons have got into an atmosphere where it predominates. If we, however, get rid of one of oxygen in CO_2, *i.e.*, reduce CO_2 to CO, or carbonic oxide, we obtain the "Wood-gas" now used in New York. This is done "by forcing the gas through red-hot charcoal" (abs. *Chem. Soc. Jour.*). By doing this we get rid of the one of oxygen which was in excess, and so obtain a gas which has different properties—for it is now a "supporter" of combustion. The gas needs very little purification, the dangerous, obnoxious, and otherwise objectionable products being absent. The wood is only drawn from the retorts twice a day, "to prevent too great an accumulation of charcoal," whereas coke is withdrawn each time. "53,832 cub. feet of gas are produced from a cord of wood, costing 5·25 dollars (about 21s.) per cord of 3,300 lb. The quantity of gas alone is thus *four times as great* as that which can be obtained from coal, costing an equal sum per ton as wood does per cord." In New York, wood-gas is sometimes mixed with an inferior coal-gas, forming what is known there under the name of "commercial," which has a great candle power.—F.C.S.



THE MAGIC WHEEL.

WE give, this week, the series of pictures of a galloping horse. We have to notice, however, that the instructions given in the *Scientific American* are erroneous. If a slit is cut exactly beneath each figure of a horse, we get a view of a horse galloping without advancing. Eleven slits should be cut (which the reader will find no difficulty in doing), at equal distances, when the horse will not only be found to move his legs, tail, &c., but to advance, as might reasonably be expected from a galloping horse. The same remarks apply, of course, to the trotting horse, in number 10, in fact, the trotting horse alone is taken from the *Scientific American*, the ten views of a galloping horse being from a series kindly supplied to the editor by Mr. Muybridge, of San Francisco, who photographed them. Twelve slits will produce the desired illusion even better than eleven, a correspondent notes, and their places are more easily measured.

THE MOON AND THE WEATHER.

IT is held by a large number of educated people that a belief in the influence of the moon on the weather is a remnant of a past and now discredited system of divination by which all the events of life were referred to the influence of the heavenly bodies. It should, however, be borne in mind that the germs of truth may be found in every system of religion and philosophy, and that the interests of truth are served better by seeking for the truth underlying any particular theory, than by denouncing it as false because it lies beyond the range of superficial observers.

In the first place, the theory of lunar influence upon the atmosphere stands apart in a great degree from the old system of astro-meteorology; for, as may be seen in the text-books containing the

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data on which predictions were founded, notably Ramesey's "Astrologia Mundi," it was the conjoined influence of the sun and planets, rather than the moon's position relative to the earth, which was mainly relied on as causing atmospheric changes, and whatever nucleus of truth the system contained was necessarily obscured and rendered almost valueless by the imperfect observation and boundless credulity of the times. And, in the second place, it must be clear to every tyro in natural science, that if it be rational and in accordance with the verifications of science, to assert that the moon's influence acts upon the waters of the ocean, it cannot be foolish and irrational to hold that the same influence affects the waters of the clouds and the air in which they float—both ponderable bodies equally subject to the laws of gravitation. So much may be taken for granted.

But the most tangible objection urged against the theory of lunar influence is that atmospheric changes are not coincident—or, if so, only accidentally—with changes of the moon, and, therefore, it is unreliable. This reasoning, however, is in itself fallacious. Those who predict changes in the weather from the moon's influence, allege that the disturbing force is felt when she is on the equator, crossing from north to south declination, or, *vice versâ*, arriving at one of the stitial colures at its extreme north or south declination. And, of course, she may or may not at such times be at the points called syzygies (the new or full moon), or at the quadratures. Thus, an atmospheric change is not necessarily to be expected at every change of the moon; but it may reasonably be when she arrives at one of the cardinal points, and it is found by experience almost invariably to occur.

But, without the knowledge of what the theory really is, the most acute critics must fall into error in judging a system whose principles they misapprehend. To give a striking instance—It is asserted by no less an authority than Professor Jevons, in his Science Primer, "Logic," that the belief in lunar influence on the weather is an illustration of "fallacy." He says:—"In one sense it is a fallacy that the moon governs the weather, because long and careful enquiries have shown that there is no correspondence between changes of the moon and changes of the weather." This reasoning clearly shows how the subject is misunderstood by those who accept the popular notion of the theory. Certainly, a change need not be expected if the syzygies and quadratures are alone regarded. The same writer says further:—"There are at least twelve new moons in each year, and changes in the weather take place in this country at least once a week on an average. It is, therefore, quite likely that a new moon and a change of weather will happen together now and then." To this the reply is that it is quite likely, for the moon usually, in the course of each week, either crosses the equator or arrives at one of the stitial colures.

And those who have made atmospheric changes a subject of special observation, are compelled to admit that the theory of lunar influence cannot be tabooed as unworthy of attention. In a number of the *Journal des Débats*, published last summer, M. de Parville, in an article on the temperature of the present year, says:—"A very long series of observations has also shown that the moon, which passes every month from one hemisphere to the other, influences the direction of the great atmospheric currents. The changes in those currents, in consequence of the prevailing moisture or dryness, are intimately connected with the relative position for the time being of the sun and moon."

Admitting that the time of atmospheric changes, taking local and climatic conditions into account, may be approximately known, it remains to be seen how far the relative positions of the other members of our solar system affect the character of the change; whether, for instance, the chemical rays of one planet, when stationary, or in conjunction or opposition of the sun, incline to heat, and those of another to cold, is a legitimate matter of investigation, always remembering that the wider the scope for surmise and assumption, the more carefully must hastily-propounded theories be tested by verified conclusions. To formulate into a system the effects of solar, lunar, and planetary aspects and motions, requires long-extended and careful observation, but, on the other hand, it must not be too hastily assumed that an error in detail proves the principle to be false—a sweeping condemnation which no logical mind should accept. When mathematical or scientific demonstration cannot be given off-hand, and when, consequently, so much depends on the even balance of the judicial faculties, scepticism is as dangerous to the interests of the truth as credulity. H. A. BULLEY.

THE PRINCIPLE OF THE VERNIER, *vide* last week's KNOWLEDGE, page 224, 11th line from bottom,

for "$x = \frac{7}{10}$ + a fraction," &c.,

read "$x = \frac{8}{10}$ + a fraction," &c. JOHN R. CAMPBELL.

SOMETHING ABOUT THE POTATO.

HOW few of us ever give any thought to the food we eat, beyond the fact of its being well or badly cooked. Very few know that the potato for years past has, amongst other articles of food, received the earnest attention of investigating chemists at home and abroad. A few of the facts resulting from their work during the last few years, I now propose to try—without going into that part of the researches which can only be of interest to the chemist, such as giving the results of analyses, or the names of the various *alcohols* which we are able to extract from the potato—and put before you as an interesting article.

I daresay some, or rather most, of you have heard of potato spirit, to obtain which, potatoes may be mashed either with mull or sulphuric acid. Mull, if anything, gives a trifle better result. In manufacturing this spirit we obtain an oil, called fusel oil, from which we can extract *nine* different alcohols, as well as other organic substances, the names of which I will not trouble you with. Besides the spirit and ether we obtain from potatoes, we may soon advantageously obtain pressed yeast. At present there is a little difficulty to be overcome in the working of the process, the difficulty being that the yield of spirit is less in proportion to the yield of yeast; but in a short time the chemists, who are working the solution of this problem out, expect to overcome this obstacle.

Another important produce manufactured from the potato is potato starch, which, by the action of acid, is converted into glucose-sugar, or syrup, which is chiefly made in America and Germany, though we have, at the same time, some important works in this country, and one quality is, from the samples which I have seen, for brewing and sweet manufacturing purposes, the purest. Owing to the large amount of sulphuric acid present in some of the prepared syrup in America, it has been found to have had an injurious effect on the health of those people who have taken the syrup, but this acid should not be present in so large an amount as this. Some samples I have tasted were perfectly free from it, and you could only taste the very sweet taste of glucose. On the other hand, other samples have really been unbearable, owing to the large amount of acid present– in one case, in the form of sulphurous acid, which leaves the same taste in the mouth as a mouthful of burnt sulphur would do, if inhaled. As glucose is one of the chief ingredients required for brewing, it is easily seen what an important substitute for malt this must be to the brewers, though a few years ago brewers would not acknowledge using it! Its use has since then become more general, so brewers are not ashamed to own to using it. Without going into detail, I will here give a general average analysis of the potato:—

Water.	Nitrogenous substances.	Oil.	Non-Nitrogenous Substances.	Cellulose.
75·77	1·79	·16	20·56	·75

A full analysis of the above, giving the names of all the non-nitrogenous substances, &c., with their percentages, would take up a good deal of space, without making the general reader any the wiser than he was at the beginning.

The next thing to be considered is the general cultivation of the potato, as made known to us by the chemists who have been studying this branch of the tuber. Generally speaking, large seed produces more large potatoes than small seed. Most people might take this for granted, without corroborating the supposition by numerous experiments, as the chemist does, for the chemist must bring forward positive results only to bear out his suppositions.

The influences of the blossoms makes a great deal of difference in the yield of the crop, as the following results show:—208 centrs. 19 lb. of tubers were obtained from plants from which the blossoms had been removed, and only 181 centrs. 48 lb. from plants not so treated. I do not think many readers of KNOWLEDGE would have thought that the difference could have been so great.

It has been found that the growth of sprouts on potatoes depends on their nearness to the apex of the tuber; if they are treated with water in sunlight, the growth increases forty or fifty times, although this not the case in the dark. Absence of light is necessary for the growth of young tubers.

Heat produces very beneficial results to the growing plant. On this account too early planting is injurious, for potatoes require the same total heat to bring out the leaf buds; so, should the plants be planted too soon, it would require, as experiment shows, perhaps forty days instead of fourteen to bring them to this stage, but it is well, at the same time, to bear in mind the fact that if they are left too late, the ground will become equally unsuitable.

Potato rot is the next point to be considered, of which there are three kinds—viz., dry, wet, and sweet rot. In the dry and wet rot it has been found that the potatoes with the maximum starch resist the disease most effectually. Potatoes grown on moist soils and soils containing much organic matter are most liable to the disease. The starch in diseased potatoes is yellow, but can be used for manu-

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facturing a second quality of dextrin. In dry rot the tuber is loose and spongy, coated on the outside with mould. The disease is originally due to Bacteriæ, and another parasite is often present at the same time. Wet rot, I forgot to say, differs from dry rot in this respect, the interior of the tuber is partially liquid and the outside coated with mould. During rotting, the potato loses half its nitrogenous constituents and the whole of the sugar.

The Sweetwort is formed by a parasite called *Mucor mucedo*. The mycelium of the parasite travels to a certain distance inside of the potato, and then disappears. Bacteria rapidly destroys the plants. There is another parasite, called *Aspergillus niger*, which produces a similar result. It seems rather uncertain as to whether cane sugar or glucose is formed: some chemists say the one, others the latter.

We all are familiar with frozen potatoes, but not many of us know in what manner the frost affects them. Frost has the effect of doubling the amount of sugar in the tuber, the starch diminishing in proportion; part of the protein passes from the congulable to the soluble form.

To prevent rot in potatoes, the tubers, whether sound or diseased when taken from the ground, are left in a weak solution of calcium chloride—one part to a thousand of water—for half-an-hour. They are then transferred to a soda solution of the same strength, after which they are washed with clean water and air dried. Half kilo of calcium chloride and the same amount of soda is sufficient for 250 kilos of potatoes. A kilo is a little over 2 lb. 3 oz.

Even though potatoes are badly diseased, they are still useful for cattle food. They are best cut up and boiled or merely scalded, mixed with chopped straw or chaff, and stored in narrow trenches covered up with clay. So stored, they will keep for many years. If steamed, they should be kept in casks, as there is more adhering moisture. If it is not convenient to heat them as above, they should at once be sent to the distillers.

With regard to the best kind of potato for yield and quality, there seems to be a good deal of difference in opinion, as the following examples will show. 1. For quality and quantity: Champion, Richter's Imperator, Eos, &c. 2. Richter's Imperator for yield is far the best; is of a hardy nature, smooth, and the stalk is tall and straight. Next in order are Violet, Victoria, Irlachin, Richter's Snowrose, &c. 3. For starch and yield: Eos, Aurora Alcohol; lower in yield but very high in starch, Achilles and Ceres. Richter's Imperator Early Rose, &c., very heavy yields. The above forms three opinions of three chemists; they all seem to agree that the Imperator is the largest yielder.

I think I have now put before the readers of Knowledge the most important points in the potato which have received the attention of chemists during the last few years. There is one more branch of the researches on which a few words may be said, and that is on a few of the results obtaining through *manuring*. At the same time it must be mentioned that experiments on large scales are still being carried out with respect to numerous manures on different kinds of ground.

The most successful way of manuring is to partially dig the manure into the ground. The application of nitrogenous and phosphate manures without farmyard manure is much less to be trusted on a poor soil than on a rich one. Manuring on fertile soil is not productive of any great gain. Sheep-dung produces a yield of 50 per cent. higher than any other artificial manure, and 60 per cent. higher than if no dung be applied at all. Extra supplies of ammoniacal and superphosphates produce no increase. Compost has the same effect as if the land were unmanured, except that there is an increase of starch. The unmanured plots are highest in disease. A good mixture is Chili saltpetre and Baker's superphosphate in the proportion of one of the former to two of the latter mixed with stable manure. Bone meal, one author says, increases the weight of product, and it is also very likely that the whole plant likewise increases in weight. There is another substance which has been tried, viz., peat, and good peat is found to give even better results than good stable manure.

If I have interested, and at the same time shown the readers of Knowledge that there is a good deal of matter for reflection, even in the use of every-day articles of common food, such as the potato, I am satisfied. F. C. S.

THE PURPLE OF THE ANCIENTS.

From the interesting "Notes on the Purple of the Ancients," by Edward Schunk, Ph.D., F.R.S., the following abstract, which may be interesting to the readers of Knowledge, is taken:—

After briefly referring to the works of Pliny and Aristotle as containing accounts of the shell-fish used in their days for the purpose of dyeing, and also of the processes incidental to same, the author proceeds to lay before the Fellows of the Chemical Society the history of this interesting dye as known to modern authors and investigators.

A shell-fish from which this dye can be extracted is found on our own coasts, Cole, in 1683, having discovered on the coasts of Somersetshire and South Wales a species of fish which, by proper treatment, could be made to dye linen and silk a *fine purple*. Reaumur, in 1710, discovered the same animal on the coast of Poitou.

The shell-fish is known to conchologists by the name of *Purpura capillus*.

The results of the various observers—Cole, Reaumur, Duhamel, (1736), and Bancroft (1803), may be summed up as follows:—

1. The colour-producing secretion, which resembles pus in appearance and consistence, is contained in a small whitish cyst or vein, placed transversely under, but in immediate contact with, the shell, and near the head of the animal.

2. This pus-like matter, either diluted with water or undiluted, on being applied to bits of white linen or calico, and exposed to sunlight, rapidly changes its colour, passing from yellow through light green, deep green, and "watchet-blue," to purplish, red or crimson. While these changes are in progress, a strong odour like garlic or asafœtida is given off.

3. To produce this change of colour, the light of the sun is essential. It is effected more rapidly by the direct action of the sun's rays than by that of diffused light, but it does not take place in moonlight, or in artificial light. If the linen or other fabric to which the secretion has been applied is kept in the dark, it remains unchanged, but when exposed to the sun it becomes purple, even after the lapse of years, though a little more slowly than at first. The metamorphosis which the change of colour indicates is not sensibly promoted by heat. It proceeds in a vacuum and in hydrogen or nitrogen gas as speedily as in air, on exposure to light. It seems to be hastened by the addition of certain chemical reagents, though these cannot replace sunlight, which is indispensable for initiating the change.

4. The colour produced is remarkably stable, resisting the action of soaps, alkalies, and most acids; being destroyed by nitric acid and chlorine.

Bizio, in 1855, experimented with the *Murex branduris;* the conclusions deduced were similar to previous observations. A. and G. de Negri, in 1875, experimented with the *Murex branduris* and *M. trunculus*.

They state that the purpurogenic secretions of these two species are not identical, the secretions of the former being photogenic, *i.e.*, not coloured when protected from the action of the light, whilst the latter becomes violet, even in the *dark*, by the sole action of the air.

Observers on the subject say that the action of light is indispensable in the action of colouring matter in mollusca.

The author conducts his experiments with the *Purpura capillus*, which he found on the rocks at low water near Hastings. He worked them up immediately after collecting, as it is necessary to use only live animals.

Breaking the shell, he easily found the pale yellow vein at the back of the animal, which may easily be cut out for examination. Putting some of the yellow viscid secretion on linen, the following changes occurred: the colour changed from yellow to green, and then to purple—emitting during the process the peculiar odour referred to in No. 2 of the summary of previous observations. The author finds that on linen, at least, the colour is not such a very brilliant purple, though it seems permanent, resisting the action of a strong soap liquor.

A temperature of 100° had no effect, either in retarding or hastening the formation of colouring matter.

Besides the above, the author found that sunlight was *not* the only agent which would bring out the deep colour, for hydrochloric acid likewise possesses this property on the veins of this remarkable shell-fish; but he thinks, at the same time, that it is open to doubt whether the action in this case is similar to that produced by insolation. It is interesting to watch the changes which occur in colour through a microscope, and if any of the readers of Knowledge desire to pursue the interesting investigation of the properties of this shell-fish, I shall be glad, at any time, to furnish them with particulars of the author's observations on this point.

Although we have, in the Old World, long since left off using the shell-fish as an article for dyeing, it is still used, to some extent, in America for this purpose, more especially on the Pacific coast of Nicaragua and Costa Rica.

The following is one of the interesting accounts, given in the paper, of the process as practised in Central America:—

"Some of the cotton fabrics manufactured by the Indians are very durable, and woven in tasteful figures of various colours. The colour most valued is the Syrian purple, obtained from the murex shell-fish, which is found on the Pacific Coast of Nicaragua. This colour is produced of any desirable depth and tone, and is perma-

nent, unaffected alike by exposure to the sun and to the action of alkalies. The process of dyeing the thread illustrates the patient assiduity of the Indians.

"It is taken to the seaside, when a sufficient number of shells are collected, which, being dried from the sea-water, the work is commenced. Each shell is taken out singly, and a slight pressure upon the valve which closes its mouth forces out a few drops of the colouring fluid, which is then almost destitute of colour. In this each thread is dipped singly, and after absorbing enough of the precious liquid, is carefully drawn out between the thumb and finger, and laid aside to dry. Whole days and nights are spent in this tedious process, until the work is completed. At first the thread is of a dull blue colour, but upon exposure to the atmosphere acquires the desired tint. The fish is not destroyed by the operation, but is returned to the sea, where it lays in a new stock of colouring matter for a future occasion."

The author had some of this fabric sent him, but the colour did not quite realise his expectations. The yarn and calico were of a *dull purple*, which is rather different from the general idea of the famous Syrian dye. They were harsh to the touch, and emitted a peculiar smell. A shell was sent with the articles; it resembled the *Purpura capillus* of our coast, but is much larger, belonging, the author says, to the *Purpura patula* of conchologists. The author informs us that in the process which the natives who dyed the goods used (samples of which he received), the whole animals were picked out with a pin. So we see there are various ways of making use of the animal.

F.C.S.

METEORIC ORGANISMS.

BY CARL VOGT.

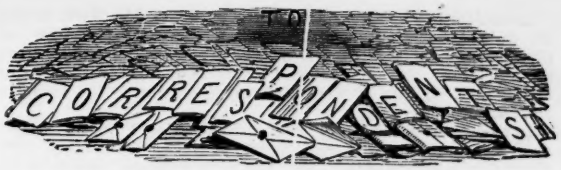
THE organisms in meteorites (chondrites), announced by M. Hahn, have no existence; what has been described and drawn as such, results from crystalline conformations, which are absolutely inorganic, None of these imagined organisms has the microscopic structure belonging to the organisms with which they have been associated. In particular, the asserted sponges do not show the structure either of existing or fossil sponges; the so-called corals do not show that of polops or anthozoa; and the imagined crinoids do not show the structure of known crinoids. The observed structures are due to an opaque crust, or result from optical illusions, caused by an incomplete method of conducting microscopical researches. Apart from pulverulent masses, metallic substances, and non-crystalline encrusting matter, ordinary meteors are composed of crystalline elements collected into granules, as is shown by their disaggregation, either by wearing down, or by the use of acids. —*Les Mondes.*

INTELLIGENCE IN ANIMALS.

AN acquaintance of mine, Mr. H., is the possessor of a cat that is a great admirer of birds as an article of food, and he has more than once devoured a chicken, for which he received condign punishment. A short time ago Mr. H. placed a number of eggs under a duck, but the only result was a single, weakly duckling. This my friend laid on the fender before the kitchen fire, and tended the little creature for some time. Eventually he put it into the garden, in order that it might get its own living, and then resumed the sowing of some seeds. On looking round in a few minutes, he saw the cat seize the duckling and dart into the house. Mr. H. followed with the intention of punishing the thief, but he found that the cat had set the duckling in its accustomed place on the kitchen fender, and was caressing and carefully licking some dirt stains from the down of his new friend. Mr. H. frequently afterwards took the duckling into the garden in the presence of some of his neighbours, and the cat invariably carried its little favourite back to the kitchen fire. I think this is a remarkable instance of the suppression of a strong instinct on the part of the cat, and may it not also have learnt a lesson of kindness?

STUDENT.

THE EFFECT OF SEWAGE ON OYSTERS.—Many people have complained of feeling ill after eating what they have afterwards thought were stale oysters. But the microscope has now shown that this has been caused by "germs" present in the liquid of the oyster. These "germs" are similar to those found in sewage—hence the deduction that the presence of sewage pipes near oyster beds has a poisonous effect upon the oysters "especially the northern side of Dublin Bay" (Abs. *Chem. Soc. Jour.*—C. A. Cameron). When the oysters were submitted to analysis it was found that the fact asserted by the microscopist was fully corroborated. This now accounts for the reason of people complaining that the oysters were stale.—F. C. S.



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**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

VEGETARIANISM.—COMMUNICATION WITH THE MOON.—LIGHT AND LANTERN.—CHOANITES.—THE NAUTILUS.—CELESTIAL MAPS.

[207]—Those amiable fanatics, the vegetarians, seem determined to force their way into the columns of KNOWLEDGE; and there, as elsewhere, to insist upon every one, high and low, rich and poor, young and old, adopting their panacea on pain of being branded as idiots for rejecting it. Now, I have no particular faith in the "whole hog" system, and believe that I may eat a cutlet without tomato sauce; and even drink a glass of claret or sherry without imperilling either my longevity, my digestion, or my intellectual powers. May I express a hope, then, that some of those who would send all mankind back (like Nebuchadnezzar) to graze, will listen to what Dr. Wald says, in Carper's *Quarterly Journal*, as quoted by Dr. Drysdale in the *Echo* newspaper. "Some prisoners in a castle at Waltenburg, who had a mortality from 1846 to 1851 of 48 per 1,000, in 1852 had a mortality of 380 per 1,000, the causes of death being diarrhœa, scurvy, and dropsy. Dr. Wald, on investigation, found that, as the potato crop had failed, these prisoners, on theoretical chemical grounds, had been fed principally on white and grey peas and lentils with bread. The mortality increased terribly on this diet, and more leguminous food was supplied, so that in 1854, beans were given five times a week, when the mortality still increased—whole troops of the prisoners becoming blind and dropsical. Wald at once dropped this theoretically nutritious food, and gave milk, bread, rice, meat, and coffee, and the normal mortality soon reappeared." I have eaten meat for upwards of forty years, and certainly have not spent twopence in medicine during the last ten of them. Moreover, I will do the hardest day's walking shooting, rowing, riding, or thinking, against any man whatever of my age who has lived upon potatoes and turnip-tops for an equal time; with the moral certainty that, in sporting slang, I should "beat him off his head."

I have no book of reference at hand, but in connection with letter 206 (p. 233), would say that, unless my memory is more than usually at fault, it was Gruithuisen, of Munich, who proposed to communicate with the Selenites by the erection of the familiar diagram to the 47th proposition of the First Book of Euclid, on some extensive plain or desert on the earth's surface.

Mr. J. W. Stow (query 169, p. 234) should obtain vols. xxxii. and xxxiii. of your contemporary, the *English Mechanic*, running through which he will find a series of articles by Mr. Lewis Wright, under the title of "Optics with the Lantern," which supply the very thing he requires.

Query 171 (p. 234) may be answered by saying that the fossil choanite must have been nearly related to the modern family of Alcyonidæ, to which the common organisms, called "Dead Men's Fingers" by the fishermen, belong. It has no affinity with the sea anemones at all. It is the polypidom of a zoophyte. I have seen a large number of choanites in my time, but "the spiral worm round the body" is something new to me.

Assuming that by "gut" in Query 177 (p. 234) Mr. Webb means the siphuncle of the nautilus, the most plausible explanation yet given of its use is that by Mr. Searles Wood—that, passing right through all the closed and isolated chambers of the shell, it serves to maintain the vitality of the entire organism during the animal's certainly long life.

In answer to Query 184 (p. 234), Middleton's "Celestial Atlas" is, or was, published by Jarrold & Son, of Norwich, and costs, with the "Companion," about 15s. Gall's is, I fancy, published by Gall & Inglis, who are Scottish publishers, I forget whether in Edinburgh or Glasgow. The price of this I have forgotten too.

In "Nights with a three-inch Telescope," in Fig. 5 the smaller star should be at the bottom. In Figs. 6 and 7 the rings of Saturn and the disc of Jupiter are *grey* in the engraving. The rings and all Jupiter's disc not covered by belts should be *white*. The small star beneath ε Persei, also in Fig. 8, is to the *left* of a perpendicular let fall from its primary, instead of slightly to the right of it.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

CHEAP TELESCOPES—SUNLIGHT ON FIRES—TEMPERATURE OF INTERSTELLAR SPACE—ICE—TIME OF THE GLACIAL EPOCH—HEAT FROM THE STARS.

[208]—If "R. I. P." (query 135, p. 188) will take my advice, he will make no attempt to construct any telescope, cheap or dear, himself. At any rate, five guineas would be the lowest price at which he could obtain anything whatever worth looking through, and that would not be a weariness to the flesh. I presume that by 2¼″ and 3″ he means 2¼ and 3 *inches* respectively; but " really signifies seconds of arc, and, legitimately, nothing else whatever.

"N." (query 136, p. 188) may rest assured that it is a mere vulgar superstition that bright sunlight interferes with combustion. It renders flame almost invisible, and so gives an ordinary fire the aspect of going out; but anyone who will take the trouble to shut the shutters will see at once that the seemingly decaying fire is burning as brightly as ever. As for cigars, I can not only always keep one alight, but never, to my recollection, has one gone out, in brilliant sunshine.

In answer to query 139 (p. 188) estimates of the temperature of interstellar space not unnaturally differ considerably. Hopkins's was − 39·5 deg. centigrade (No. 15, "Monthly Notices," Vol. XVII., p. 192, Fownes estimated it at −50 deg. cent., and Pouillet as low as −142 deg. cent. These are all mere deductions from theoretical considerations, as of course the temperature of space has never been actually measured.

One single fact will suffice to show that ice (Query 140) does not vary in volume, as other solids do, with variation of temperature; since it *contracts* during liquefaction.

There can be very little doubt that Dr. Croll's Theory (Query 141) is substantially correct. No other theories exist which are worthy of scientific attention. Adhémar's is worthy of Jules Verne; while Colonel Drayson's—well, his is worthy of Adhémar.

It may interest "Student" (Query 161, p. 211) to hear that Mr. E. J. Stone, the present Radcliffe Observer, in 1869 determined the heating effect of Arcturus to amount to 0.00000137 deg. Fahr., about equivalent to that radiated by a 3-inch Leslie's cube of boiling water 400 yards off! Curiously the heat from *a* Lyræ is *less* than this; in fact, it is only equal to that emitted by a 3-inch cube of boiling water at a distance of 600 yards.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

INTELLIGENCE IN ANIMALS.

[209]—A pony belonging to a gentleman in this neighbourhood showed great unwillingness to carry any one on his back. One day he was being ridden by a young lady; he seemed to be very spirited, and galloped along the drive at a good pace for some distance, when he suddenly stopped, stretched out his neck and threw the lady over his head.

For some time after this he was not ridden, but the fright gradually wore off, and his back was once more called upon to bear the burden, until the following incident occurred, after which I don't know what became of him. He was trotting briskly along the road with his young master on his back, when, on coming to a shallow pond on the right-hand side of the road, he abruptly *turned at right angles*, took a step or two into the pond, then came to a stand, throwing his rider into the centre of it. He stood for a moment and looked at his unseated rider, then trotted quietly home.

I don't know whether this will be considered *reasoning* or not. The pony seems to have thought that if he got off for so long by throwing his rider on the ground, he might get off for a longer time if he threw him in the pond; certainly he must have considered it more disagreeable to be thrown into the pond than on to the ground, seeing he turned into the pond to accomplish it.

Edinburgh. J. B.

ASTRONOMICAL CURIOSITY:—EVOLUTION.

[210]—I have seen somewhere the following paradox, as I suppose I must call it. Granting (1) that the stars are on the average as bright as our sun, (2) that their number is practically infinite, and (3) that light travels without loss (all of which premises appear to me perfectly reasonable); it follows that the sky should *always* be as bright as if full of suns, and square suns, so as to fit closely. For a star's greater distance is compensated by the greater number that can shine near together, and the theory supposes that in any direction we should reach stars sooner or later. This result is, however, so different from reality, that I should be glad to be shown where the fallacy lies. [In our next.—ED.]

I venture to point out to Mr. Donbavand (Letter 183), first, that biologists should no more be expected to account for the origin of any "first-born animal," than that astronomers should be called upon to point to the origin of the nebulous globe, from which our solar system is derived, or theologians to explain whence came the material from which it was made (since we are told that "created" means "set in order"). Secondly, Pasteur, or any one else, has never disproved the possibility of "spontaneous generation;" all he proved related to his particular experiments, and similar ones. How do we know, for instance, that, *even now*, the process is not going on in our ponds and ditches, side by side, if you like, with the known modes? It is not possible to *disprove* a thing like this, and the *probability* of it must remain a matter of opinion. Thirdly, is it more astonishing that a fish should develope into a reptile by continuous slight modifications, in course of ages, than that a tadpole should become a frog, in the same individual, in a few hours? I think Mr. Donbavand should read Hæckel's "History of Creation," if he still feels interest in the matter.

LEWIS ARUNDEL.

LIQUIDS AND THEIR VAPOURS.

[211]—At what Mr. M. Williams has described (see KNOWLEDGE, No. 8) as the "critical temperature" of liquids, the mysterious disappearance of all visible distinction between the liquid and its vapour, is much misunderstood by many persons, who imagine that, when this temperature is arrived at, "the whole of the remaining liquid is suddenly converted into vapour."

We know that liquids expand (becoming lighter) as their temperature is raised, and, at the same time, the density (and elastic force) of their vapours increases at a very rapid rate, so that we might reasonably expect that, after a certain increase of temperature, the densities of the liquid and of its vapour would become equalised, and at this point, of course, the eye could no longer perceive any distinction between them. This, then, is "the critical state," which thus loses its "mystery." [Not quite.—ED.]

No experiments, so far as I am aware, have been instituted with the object of testing this view. But some of those by Cagniard de la Tour (as in the following table) plainly point to the result I have mentioned above as regards "ether" (See Miller's "Chemical Physics," 1867, p. 366).

Temperature Fahrenheit.	Pressure in atmospheres of the vapour of ether. Volume of liquid = 7; of vapour = 20.	Volume of liquid = 3½. of vapour = 20.
257°	10·6 atmospheres.	14 atmospheres.
302°	18 „	22·5 „
324°·5	22·2 „	28·5 „
369°·5	37·5 „	42 „

On increase of temperature, as above, = 67°·5 (from 257° to 324°·5), the pressure (in *each* column) is *more* than doubled; whereas, with same increase of temperature = 67°·5 (from 302° to 369°·5), the pressure is *more* than doubled (by 7½° atmosphere) in the first column, while in the second column it is *less* than double pressure by three atmospheres. Hence, the conclusion that in the latter case, *all* the liquid having passed into vapour, the full pressure was not attained; whereas, in the former case, where some of the liquid still remained (though imperceptible), the tension continued to increase from addition of vapour, density of *both* liquid and vapour increasing *equally* with increase of temperature.

It would be desirable, if possible, to ascertain (by spectroscope or transmitted polarised light) if any "molecular change of arrangement or of motion" could be detected in these different states of liquid and gas co-existing at the same density.

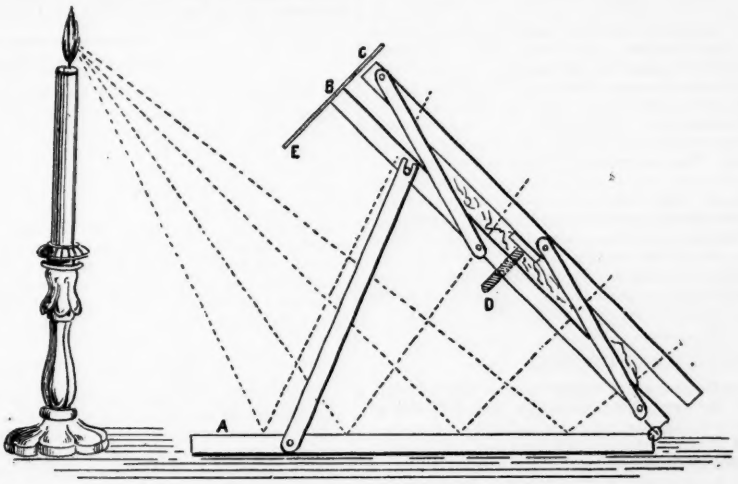
HENRY HUDSON, M.D.

TATTOO MARKS.

[212]—I have asked myself the question which your correspondent now asks (166), and I have answered myself in this manner:—The atoms inserted by tattooing are quite foreign to those constituting the human body; they do not form and disperse with them, but must be removed as mechanically as they were inserted. They were dead and foreign atoms when introduced, and so remain, uninfluenced by the living electrical changes that surround them. Might I compare them to a boulder thrown into a running stream? If the stream were strong enough, it would carry away the boulder. If an abscess were to occur where the tattooing took place, the powder would be carried away.—J. J. A.

BOTANICAL CONTRIVANCE.

[213]—Those of your readers who are students of botany may find the contrivance, of which I send you a diagram, useful for obtaining a correct outline of the specimens which they wish to draw. It was made for a friend, who wanted a means of copying flowers, &c., the exact size of nature. A is a looking-glass laid flat upon the table, B and C are wooden frames, each holding a square of plain glass. The flower to be drawn is laid between the glasses, which can be kept at any distance apart by means of the parallel links on each side, and the screws at D. A piece of paper is laid upon the upper glass, and by the light of a candle reflected from the mirror, the shadow of the flower is projected through the paper, and its outline can be easily traced. The paper can then be removed, and



BOTANICAL CONTRIVANCE.

the shading and colouring copied from the object, which is held in the same position between the glasses. A skilful draughtsman may despise such aid, but it has been found useful for drawings aiming rather at correctness of shape and size than at artistic effect. The shadows will, of course, be very slightly larger than the object. The machine might also be useful to designers of Christmas cards, or floral patterns of any kind. It can easily be made with a common looking-glass and two picture frames, and a few pieces of brass wire. A cardboard screen should be placed at E to prevent the light from falling directly upon either side of the paper. Everything must have a Greek name nowadays, so we call it the SKIAGRAPH.

COLOURS OF STAMENS.

[214]—I have read Mr. Grant Allen's interesting and clever paper in the *Cornhill Magazine*, in which he considers yellow to be the original colour of flowers, founding his theory on the fact that most flowers have yellow stamens. This is scarcely borne out by an examination I have made of twenty-three flowers in a conservatory, of which I found eleven had yellow, seven white, and five red or pinkish stamens. I presume Mr. Allen in speaking of stamens means the filaments and not the anthers only, which are certainly almost always yellow. As Mr. Allen is a contributor to KNOWLEDGE, he may be disposed to give further explanations, and to furnish more details in that periodical. Some orders of plants appear to have nearly always yellow stamens. In others they appear to come near the colour of the throat of the corolla. It is, however, too early in the year to investigate the subject thoroughly. T. HOWSE.

MARINE BOILERS.

[215]—With reference to "Crankshaft's" letter, 114, p. 167, I can tell him that in marine boilers it is not merely the incrustation of salt which prevents heat passing from the furnace or tubes to the water. As a rule, marine engines are surface condensing, and the grease used for lubricating the cylinders passes with the steam into the condenser, and thence into the boiler. It here, apparently, enters into chemical combination with solids mechanically held in suspension in the water, and deposits this upon the fire-box and tubes in a thin, hard scale. This is a most extraordinary non-conductor of heat—so much so, that I have known boiler-plates almost heated to redness without maintaining the steam higher than 100 lb., the scale being only as thick as one's thumbnail. If this scale be taken out, pulverised, and rubbed on the hand, and water poured on it, the water will assume a spheroidal shape, and run off without wetting the dust, showing the presence of grease in large quantities.

Another experiment is—spread some of the scale, when moist, upon a tin plate, and place a drop of water on same, and hold the same over a lamp. A very long time will elapse before evaporation takes place. Tilt the plate so that the water runs on to a portion which has not been covered by the scale, and the water will at once evaporate.

The above will perhaps be more readily noticed as occurring with water containing silicate of lime, or lime in some shape.

Letter 115, p. 167.—Undoubtedly there is a tendency to vacuum on the following side of a screw-propeller when revolving; this, however, does not affect the *power* of the steam-engine, but the *speed* of the vessel. The former depends upon the pressure of the steam and the speed of the piston. The latter depends upon the slip of the screw-propeller, *i.e.*, as to whether the propeller is passing a body of water equal to its own pitch. The freer the access for the water to approach the propeller, the greater the quantity of water passed, the less the amount of slip, and the greater the speed of the vessel. Hence, although in designing vessels for screw propulsion it is, of course, necessary to study to a certain extent the form of the bow, it is far more important to have a clean run aft.

Letters 114 and 115 contain questions of deep import to engineers, and you would do great service to the profession if you could publish what information you can obtain upon these points.

Thanks for answers to my former queries on "Gravity." Am not quite sure I agree with one small part, but have not yet had time to go fully into the matter.—Yours truly, $\frac{W}{g}$.

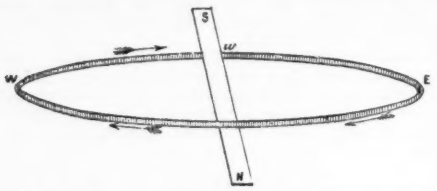
MAGNETIC NEEDLE.

[216]—Referring to query 90, p. 123, to say that a magnetic needle turns to the north, owing to the northern terrestrial mag-

netism being of an opposite name to that magnetism which resides at the north-pointing end of the needle, and, therefore, attraction ensues, would be little to W. H. P.'s purpose. He desires, most probably, to know how the terrestrial magnetism got there.

It seems to me the following view (touched upon in some text-books) is the most satisfactory:—Whenever heat flows in a closed circuit, there is a flow of positive electricity with it. If, therefore, a positive current passes in the direction indicated by the arrows in Fig. 1, the piece of soft iron would become—so long as the heat or electricity circulated—an electro-magnet. If to the upper end, marked S, of the soft iron, we were to present that end of a magnetic needle which pointed southward (geographically speaking), we should find that would be repelled, while the north-pointing end of the needle would be quite violently attracted.

The earth, during her daily rotation, offers successively parts of her surface to the warming action of the sun, and a slight consideration will show that this is practically the same as heat continually passing round the earth in a westerly direction, therefore well representing a closed circuit. We



have then all that is necessary for the state of things above noticed. The wire (Fig.) E.W.w. may well show or represent the surface of the earth exposed to the warming action of the sun (for the current will be in the same direction, viz., E.W.w.), the bar, SN, the iron contained in the earth. This iron becomes converted into a huge electro-magnet. We should then have towards the northern part of the earth the same-named magnetism as at position S in the figure. The behaviour of a needle under these circumstances we have shown; so a needle on the earth will act in precisely the same manner—that is to say, its N-pointing pole turns to the north, because the different-named magnetisms (if I may use the term) attract. Strictly speaking, there is a fourfold force acting, two on either pole of the needle, one pulling, the other pushing, not in the sense of moving merely directive; but, in order to simplify, we have neglected the other three. Their action needs no explanation, for they are all additive to the result.—G. F. J.

THE DESCENT OF MAN.

[217]—Mr. Donbavand's method of criticism, regularly adopted, would cripple scientific inquiry. He asserts the first difficulty of the evolution theory to be its foundation, complaining that it cannot give an exact definition of the period and the form in which the first living organism appeared upon our planet. Now I submit that a similar argument could be advanced, and with equal force, against the fundamental facts and theories of almost every branch of knowledge. The chemist bases most intricate calculations upon a hypothesis of atoms and molecules, of which not one has ever been seen isolated, weighed, or defined. The electrician speaks habitually of magnetic fluids which have never tangibly revealed themselves. No definite origin can be assigned either to molecules or to magnetism. Instances of this kind might be multiplied *ad infinitum*.

Science cannot and does not, as yet, pretend to any knowledge of the beginnings of law, matter, or life. Its present task is to procure and to sift evidence, to arrange and to explain known facts, and thereupon to formulate certain theories which shall enable us, reasoning by analogy, to acquire a more correct and comprehensive view of the subject of inquiry.

Darwin and his followers are stated by the highest authorities to have effected much in this direction. But, in any event, I contend that there is no scientific reproach to their theory in the circumstance that it fails to demonstrate the absolute origin of life. It is no evidence against the continued straightness of a line that our vision cannot extend to its extremities. E. Burke.

"A GLIMPSE THROUGH THE CORRIDORS OF TIME—LIGHT AND HEAT WAVES."

[218]—A writer in *Nature* calls attention to a paper published by Kant, when he was thirty years of age, in which he states, "If the earth were a perfectly solid mass, without any liquid, the attractions of the sun and moon would not alter the rate of rotation round the axis . . . . If, however, the mass of a planet includes a considerable amount of liquid, the attraction of the sun and moon, by moving this liquid, impress upon the earth a part of the vibrations thus produced. The earth is in this condition." He then goes on (says the writer) to state that the moon produces the greatest effect and the tide running round the earth in a direction opposed to that of rotation. "We have here a cause on which we can count with certainty, incessantly reducing this rotation by as much as it may be capable of." . . . "When the earth steadily draws nearer and nearer to the end of its rotation, this period of change will be completed when its surface is, relatively to the moon, at rest, *i.e.*, when it rotates round its axis in the same time in which the moon revolves round it, and will, consequently, always show the same face to the moon. . . . If the earth were entirely fluid, the attraction of the moon would very soon reduce its rotation to this minimum. Herein we at once see a cause why the moon always shows the same face to the earth. . . . From this we may conclude with certainty that when the moon was originally formed and still fluid, the attraction of the earth must in the manner above described have reduced the speed of rotation, which then, in all probability was greater, to the present measured limit."

The writer considers that Kant had given a glimpse through the corridor of time a century earlier than any of the authorities mentioned by Professor Ball.

Do light and heat travel at the same pace? Do the waves of light and heat coincide?—which, I suppose, may be only another way of asking the same question.—A. T. C.

[Light waves are, for the most part, heat waves, and *vice versâ*, though the luminous effect of different ether waves is not proportional to the heating effect. Of such waves we may say that they do not coincide, being identical. The same things cannot coincide, any more than a horse can run a dead heat with himself.—Ed.]

PLATING ALKALOIDS.

[219]—Mr. Lewis Arundel has failed to answer the point in query 152 that I wanted elucidating, viz., how to plate on iron. Having tried silvering on iron in the wet way, first coating with copper, and failed in this, I am anxious to try nickel. In Supplement II., Watts' "Dic. Chem.," the neatest method of nickel-plating on copper and brass is given. With regard to the letter on alkaloids, Watson Smith (Bericht's "Deut. Chem. Gesellschaft," xii., 1,420) gives antimony or bismuth trichloride as a test for several alkaloids, including aconitine, which gives a bronze-brown colour. Again, last year (Thresh gives in "Pharm. J. Trans." [3], X., 809, a method of estimating the quantity of this, and of other alkaloids present, by precipitation with iodide of potassium and bismuth.—C. T. B.

THE HEALTH OF NAVVIES.

[220]—After reading your abstract from the *Times* on the above, it occurred to me that M. Colin might now very advantageously try what the effect the betel nut would have in warding off the malarias, &c., contracted in marshy grounds. I think Johnson, in his "Chemistry of Common Life," informs us that the betel chewer, whether native or stranger, has an invaluable treasure in the use of this narcotic, which is a perfect safeguard against fevers, agues, and all other maladies incidental to marsh life. If this is the case, is it not worth a trial during the extensive canalisation soon to be done in France? M. Colin, should he try it, would be able to see if the effect of betel by itself is the same as when chewed with the betel pepper leaf, and quicklime. F. C. S.

SCIENTIFIC PARADOX.

[221]—The difficulty of "Eclecticus" (p. 202) may possibly arise from his conception of the manner in which the pressure of a gas or vapour arises. Liquids have a tendency to give off vapour to a greater or less extent. This tendency varies with different liquids, and in the case of the same liquid, varies with differences of temperature. But vapours have a tendency to condense into the liquid form dependent upon their nature and the conditions to which they are exposed. When these two antagonistic influences are in equilibrium, a state of stability arises. Now, although there may be an atmosphere of ether-vapour present, water will continue to evaporate either until the tendency of the water to vaporise is counterbalanced by the tendency of the water-vapour to liquefy, or until no liquid water remains. It is evident the ether-vapour takes no part in producing this equilibrium, merely retarding the formation of vapour, and going to increase the total pressure.

It may be added that the total pressure exerted in the case of the vapours of ether, alcohol, and water is very much less than the sum of their individual tensions. This only holds good for liquids which do not dissolve in one another. W. S. C.

ALUMINIUM.

[222]—With reference to query 157, I give the following method of extracting aluminium, as it may be of general interest to your readers of KNOWLEDGE:—

A mixture of ground aluminous clay (ordinary clay, but of a good quality, and soda ash (carbonate of sodium) are heated in a furnace, aluminate of soda and silico-aluminate of soda being formed. The fused mass is then broken into pieces and thrown into an iron tank containing water; the mass is frequently stirred, and finally allowed to settle.

The aluminate of soda (being soluble in water) is dissolved, while the silico-aluminate of soda (being insoluble in water, sinks to the bottom of the tank, with any peroxide of iron that may be present in the clay.

The liquid is then drawn off, and carboxic acid gas passed through the solution. This decomposes the aluminate of soda, forming carbonate of soda and pure alumina hydrate. Thus:—

$$\underset{\text{Aluminate of Soda.}}{2\,AlNaO_2} + \underset{\text{Carbonic Acid Gas.}}{CO_2} + \underset{\text{Water.}}{3\,H_2O} = \underset{\text{Carbonate of Soda.}}{Na_2CO_3} + \underset{\text{Alumina Hydrate.}}{2\,Al(OH)_3}$$

The alumina hydrate is then dried, mixed with chloride of sodium (common salt) and charcoal or coke, and formed into balls about the size of an orange. These balls are then taken and put into a vertical earthen retort, and heated to redness; then a stream of chlorine gas is passed through them. The chlorine combines with the alumina (being greatly helped by the charcoal), and forms chloride of aluminium, which unites with the sodium chloride (common salt), and distils over or sublimes as double chloride of aluminium and sodium thus:—

$$\underset{\text{Alumina}}{Al_2O_3} + \underset{\text{Common Salt.}}{2\,NaCl} + \underset{\text{Coke.}}{3\,C} + \underset{\text{Chlorine}}{3\,Cl_2} = \underset{\text{Carbonic Oxide Gas.}}{3\,CO} + \underset{\text{Double Chloride of Aluminium and Sodium.}}{2\,AlNaCl_4}$$

Ten parts of the double chloride of aluminium and sodium thus formed is mixed with 5 parts of kryolite (a double fluoride of aluminium and sodium, found in Greenland), which serve as a flux, both in a state of fine powder, and to this mixture is added 2 parts of (metallic) sodium in small pieces.

The whole is now introduced on to the hearth of a reverberatory furnace, previously heated to the required degree, when a violent reaction ensues. The dampers are then closed, and all parts of the furnace kept as close as possible, to prevent access of air. This causes the mass to completely fuse. When the action has subsided and the decomposition is completed, the furnace is tapped, and the metal and slag are run into suitable moulds. Most of the aluminium collects in the bottom of the mould. Above this are two layers of slag, the top layer being sodium chloride (common salt), the middle layer being less fusible than the top, and consisting chiefly of fluoride of aluminium, in which small globules of aluminium are mechanically held, which are recovered by pulverisation and sifting of the slag.

The following equation will show the reaction that takes place in the above reduction. Thus:—

$$\underset{\text{Double Chloride of Aluminium and Sodium.}}{AlNaCl_4} + \underset{\text{Kryolite. or Cryolite.}}{AlNaF_3} + \underset{\text{Sodium.}}{6\,Na} = \underset{\text{Aluminium.}}{2\,Al} + \underset{\text{Common Salt.}}{4\,NaCl} + \underset{\text{Fluoride of Aluminium.}}{4\,NaF}$$

There are other methods, but this gives the purest metal, and is one generally used in England and France. Messrs. J. Lowthian Bell & Co. (or Messrs. Bell Bros.), at Washington, near Newcastle-on-Tyne, manufactured aluminium on a large scale for several years, but gave it up a few years since, owing to it not paying so well as was first anticipated, and also on account of the limited demand for the metal. I think they used the above method.

Gateshead. G. W. GRAY.

DESCENT AND DARWINISM.—VEGETARIANISM.

[223]—I wish to protest against the large amount of space M. Donbavand has taken up in stating his ideas. He says that he sometimes ventures on the labour of thinking. May I venture to suggest that before he again trespasses on your space, he should think over what he means to say, and the shortest and clearest method of saying it?

E. M. (letter 170) argues that because uncooked diseased meat is apt to produce tapeworm and trichinosis, therefore we should abstain from animal food altogether. Does he not see that his own argument might be turned with equal force against himself? Because uncooked unripe fruit, if eaten at all, and all uncooked fruit, if eaten in quantity, is likely to produce diarrhœa, and so forth, therefore abstain from vegetable food altogether.—EXON.

Queries.

[185]—COLD SATURATED SOLUTIONS.—Can you inform us as to the best method of making and of ascertaining the quantity of salt present in a saturated solution at 60° F? We have tried a number of experiments, none of which are satisfactory.—F. GAUBERT and C. A. SEYLER.

[186]—THE CALCULUS.—Is a knowledge of the differential and integral calculus gained from, say, Boucharlat's or Hall's treatises sufficient to begin studying the Mécanique Céleste?—TRIA.—[It would be far from sufficient. A complete course of study of the higher mathematics must intervene, unless you are a born analyst. —ED.]

[187]—FLAME.—What are the modifications of the ordinary theory of a gas or candle flame introduced by (I think) Frankland? Perhaps you could sometimes give us an article or two on flame, explaining why one kind of burner is better than another. I take it that for a given expenditure of gas, heat + light is constant. Is there a theoretical maximum to light, and what principles require attention in our attempts to attain it?—L.

[188]—INTRA-MERCURIAL PLANET.—Leverrier formed a theory of an intra-mercurial planet, for which he surmised some three or four orbital periods. Oblige by informing me exactly what these periods were. I do not refer to Lescarbault's Vulcan, but to the prior hypotheses and periods of theory.—JOHN JONES. [The only periods that Leverrier dealt with, to the best of my belief, were calculated after Lescarbault's Vulcan was announced, though Leverrier attempted to reconcile prior supposed observations of planets in transit across the sun's face.—ED.]

[189]—EYE-PIECE.—Can you please give me an easy way to find the power of an eye-piece of 2 plano-convex lenses, both ½ inch focus, and sliding one into the other, at a distance from one another, say ¼ inch and ⅛ inch? I want this information, so I may know the power of my telescope.—J. W. C. [The lenses should be set ½ inch apart. The power is then four times that of either lens alone.—ED.]

[190]—THE PRESERVATION OF SERTULARIAS, ZOOPHYTES, SEA-WEEDS, &c.—Can the Editor of KNOWLEDGE, or a correspondent, inform me of a chemical preparation to preserve zoophytes, sertularias, and seaweeds, &c.? and is there a book on the subject, and on British and foreign seaweeds, &c.?—M. A. S.

[191]—ANIMAL LANGUAGE.—Do not the habits of swallows, prairie-dogs, &c., seem to prove that animals have some sort of language by which they can make themselves understood—at least to members of their own family?—ARACHNIDA.

[192]—THE ICE-AGE IN BRITAIN.—What proofs are there at the present time that there ever existed an ice-age in Britain?—ARACHNIDA.

[193]—ACTION OF THUNDER-STORMS.—Why are beer and milk so frequently turned sour during a thunder-storm, although they will often stand as high a temperature without being affected when there is no thunder in the air?—F. A. S.

[194]—GRAVITY.—The editorial reply to query 144 is incomprehensible, if correct. If any force, however small, will overcome the inertia of any mass, however great, what then becomes of the law, that inertia is proportional to mass? I wish to know what force is necessary to overcome the inertia of one ton weight in a direction at right angles to the force of gravity?—ZARES.—[We repeat, any force, however small. If a ton of matter were suspended as you described, or placed on a perfectly smooth table, the breath of a child would overcome its inertia. The velocity communicated to it would be very small, but you did not say how much velocity you wanted.—ED.]

Replies to Queries.

[64]—STAR NUMBERS AND LETTERS.—"A Fellow of the Royal Astronomical Society" says (p. 164, No. 8): "Stars . . . . . are both lettered and numbered in their order of Right Ascension." There should be a comma in this reply, after the word "lettered." The stars are numbered in the order of their right ascension, and Bayer lettered them roughly in order of their brightness. This answers a query by "Winter."—ED.

[71]—NAMES OF FLOWERS.—I would advise Querist 71, in sixth KNOWLEDGE, to get Bentham's "British Flora." He will find it everything he desires. Should he fail to get at the right name of a flower by means of this excellent work, I should then recommend him to send a perfect specimen of the "stranger" to the editor of

KNOWLEDGE, who, I am sure, will not consider it too much trouble to assist amateur botanists by giving names of flowers when requested to do so.—J. E. GEORGE. [We are not personally botanical, worth mentioning: better trust in the book. Though we would forward specimen to botanist.—ED.]

[130]—KNOWN LANGUAGES.—Horalacque calculates 499 languages: how many more have been made *known* since he wrote I do now know.—G. T. HARRAP.

[133]—In addition to "Paugul's" references, "Querist" will find several of Mr. Dollinger's papers in the *Monthly Microscopical Journal*, vols. X., XI., XII., XIII., XIV., XVI., XVIII.—B. J. AUSTIN.

[146]—CHEMICAL.—The reason why the O. of the air is utilised in the body, while the N. is rejected, is that the former is essential to those transformations which convert the potential energy of certain constituents of food into actual energy, while the latter is useless for that purpose. Fats and other carbohydrates, when taken into the body, furnish, by their oxidation, the actual energy which is in part transmuted into muscular power; and it is to this process of oxidation that animal heat is due. There being no other available source of O., the amount necessary for the said oxidation of the carbohydrates used as food must come from the inhaled air. It is only the heat and muscular-power-producing constituents of food—the carbohydrates—that get oxidised in the body, the nitrogenous or flesh-forming constituents, together with the phosphates and carbonates, being capable of performing their functions without undergoing the process of oxidation. Now, as the products of oxidation, CO_2 and OH_2, are exhaled, there cannot remain in the body anything that can be termed "food already oxidised." In fact, the utility of carbohydrates does not consist in their adding anything to the structure of the body, but in their producing muscular power and heat when being transformed into the gases exhaled in the breath. In starch and sugar, the O. and H. exist in the same proportions as they do in OH_2, and, as the whole of the O. and H. of those carbohydrates leave the body as OH_2, all the carbon is in a position to be oxidised by the inhaled O. of the air, and is, consequently, available to produce its full amount of force and heat.—E. M. D.

[157]—ALUMINIUM, page 211.—Cryolite (a fluoride of sodium and aluminium, which occurs in great abundance in Greenland) is powdered, mixed with half its weight of common salt, and either placed in alternate layers with two parts of sodium in a crucible, or roasted with the same quantity of sodium in a furnace. The chief advantage in using cryolite is that the costly and troublesome process of preparing the double chloride of aluminium and sodium is thus dispensed with. There was a process patented in Germany a short time ago, the main point in which consisted in fusing the sulphide of aluminium with iron; but surely other chemists thought of this process before: I did three years ago.—A GREENOCK STUDENT.

Answers to Correspondents.

*** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule* 3, *free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

J. CARGILL. Thanks for encouraging words. Your student friend who thinks we are too abstruse for the masses and too popular for scientific students is right enough; that is what we have aimed at. But he is unwise in calling it a fault. We do not write for science students, though among those who write for us are some who can teach the teachers of science students. Nor do we write for those who neither think nor care to think.—J. D. M. I have never seen unusual feats of strength done under mesmeric influence, but Dr. Carpenter gives instances of the kind. They illustrate the influence of imagination, but they have not yet been explained.—J. WILLSON. Lardner's works are not absolutely out of date, but they would suit better one who was well up in all the modern views, so as to be able to distinguish at once what is obsolete.—J. PRESTON BATTERSBY. Many thanks. Your notes are very interesting. I return them as you desire. I think you are mistaken in imagining there is any prejudice about so-called mesmeric phenomena. They are treated very fairly by some of our most eminent physiologists.—M. If, knowing what the planets are, their distances, &c., the slightest reason for conceiving that they can influence our weather could be shown, it might be worth while to make such observations as you suggest; but none can be imagined.—BRENTON. You are quite right; T. R.'s solution may be made by embodying his demonstration of the property on which it depends to all intents and purposes a solution within the limits of Books I. and II.; but with the extra matter you show to be necessary to complete it, it contrasts very unfavourably with either of the other proofs depending on books III. and VI.—ZARES. *What* series 0 to 1 do you mean?—M. J. HARDING. Orion, on the cover of my "Easy Star Lessons," is "quite too utterly awful." I had nothing to do with it. It is the binder's c nceit. Trying to correct it would be useless. In the present position of science, a work "containing a most exhaustive refutation of the Darwinian theory" must either be a very wonderful performance (as you say), or very bad. Science has not been profoundly stirred by the book. The sub-title seems open to objection, to say the least, as suggesting that the Newton of Biology is a sort of "devil's advocate."—A. G. P. We misread your letter; but our reply was not meant as you suppose. The absence of centrifugal force would produce a very slight but measurable increase of weight.—W. A. C. We have no objection; you may prefer a dog to your nearest and dearest; you must allow us, though we like Rover very much, to prefer our dear ones of human sort. To be consistent, you ought not to breathe, for, at every breath, you destroy multitudes of "creatures belonging to races below us."—WINTER. Yes; he probably smiled; but "he laughs longest who laughs last."—VEGA. Yes; but the point really is whether the classification adopted by Owen is essentially wrong. For convenience, the other may be better; but it is a merely arbitrary distinction.—H. D. P. Not about Sorghum just now; so many things wanted and promised. Sorry am unable.—GRADATIM. The loxodromic curve approaches the pole by an infinite series of circlings, though the total distance is finite, each whorl bearing to the previous one a ratio less than unity. Thus your whole question involves a paradox, as no ship could follow such a course; no wonder no ship could leave the Pole on the loxodromic spiral. Remembering that the stereographic projection of the loxodromic curve on the equatorial plane with South Pole as pole of projection is an equi-angular spiral, and that this curve approaches the pole asymptotically, you will find no difficulty in interpreting your paradox.—B. DONBAVAND. Your letters at least five times too long for admission.—J. P. SANDLANDS. South cone is up, means that this weather signal is hoisted—viz., a cone with its point downwards. Mrs. Kingsford says the teeth of man indicate that he is a frugivorous animal; others regard them as showing him to be omnivorous. As a matter of fact, he is omnivorous, whatever his dental formula may be. The other query seems answered by what is stated in our review of Mrs. Kingsford's book.—ONE INTERESTED IN "KNOWLEDGE." No one asks you to believe that man is descended from monkeys—only that he is related to them; so that what you believe is closely akin to the scientific faith, for you think monkeys descended from men. The tone of your remark that "no theory will make you believe" so and so, is unscientific in the extreme. The student of science is prepared to believe whatever facts may prove.—J. R. See p. 170, where precisely such a problem is solved.—PETER KNOWLES. I return the twelve stamps. If I "published" the work you name I would send it, but I do not publish that or any work. I did not even write it, I only revised it. If I remember rightly the publisher is Baillière, King William-street, Strand.—URSA MAJOR. A lunar rainbow is caused precisely as a solar rainbow is caused, only that the light comes from the moon instead of the sun.—V. C. C. D. I do not see how reasoning so sound and moderate as that you quote can be answered. I should imagine few evolutionists would be so extreme as those who seem to be disposed of by this reasoning.—G. T. HARRAP. You "disagree with vaccination," but your letter would disagree with our readers.—SALUTO. Yes, microscopy is as desirable in our pages as telescopy.—F. C. S. Many thanks: marked for insertion.—J. B. More rapid respiration, increasing the combustion, seems to account for the difference.—G. S. BODKIN. We point out mistakes (where we see them) frankly and without apology, because that is doing to others what we wish them to do to us.—J. MCNEILL. In issuing monthly parts, the publishers are endeavouring to meet the wishes of the public; when I note that reprints have been necessary to make up the first two monthly numbers, you will understand their price. The reprint of a newspaper always means loss of money, and the publishers have reprinted (setting up every line of contents and advertisements afresh) in some cases twice; and made up sets out of the back numbers which had been intended for volumes. Many thanks for good wishes.—C. J. BROWN. Gravity vanishes at the earth's centre, but pressure is there at its maximum. Apart from a very slight diminution, due to the greater or less rigidity at

different parts of the earth's interior, the pressure at any point within the earth is proportional to the total quantity of matter above that point, just as in the sea, where, though gravity is appreciably constant, pressure is proportional to the depth. If you ask where the force of gravity is at a maximum, I cannot answer, because I do not know according to what law density increases towards the centre. As to the other point, our circulation increased so that it became quite impossible to continue cutting the edges without unduly delaying publication. But we more than make up for that in other ways. In binding there would in any case have had to be another cutting, and most of our readers, we believe, intend to bind each volume as it is completed.—H. Fear cannot find space for paper on colours. Readers complain unless a certain proportion of space is given to original matter, and we cannot increase the total space beyond a certain range. Correspondents must not suppose we do not value letters which we are obliged to omit.—J. E. SHEPHARD. The writer of the article in question was not likely to consider the use of aconitine in homœopathic practice. It is seldom used by allopathic physicians, except as an external application.—A GREENOCK STUDENT. Clouds are formed by the condensation of aqueous vapour into small water drops, which, however, do not fall rapidly like rain drops. It can be shown that the smaller a water drop, the slower its descent through still air. However, a cloud is also constantly changing, as you suggest. The light, feathery clouds consist of particles of ice or fine snow. Colonel Ross promised other papers on blowpipe analysis, but has apparently not yet been able to find time to write them. The blowpipes made on his plan will be rusting for want of use.—TREBOR ROLAT. Short whist, played without counting honours, is, to all intents and purposes, the ordinary game with the element of chance as far as possible eliminated. It is in my opinion a much better game even than that which Clay thought would be perfect, in which the honours are counted at half the usual rate, one point for two by honours, two for all four honours. After playing the game for any length of time without counting honours, a real lover of whist feels something like shame in counting honours, or even half honours. At present we fear the star chart cannot be issued with the last week of each month, as we want one chart to appear each month. As soon as we can, the change shall be thought of. Have no space, for some time, for article on relation between mind and physical force.—DOUG. BRODIE. Your 2½ stamps received, but no paper on the Tides has yet appeared; we did not promise it for an early date, and if we had, we could hardly have kept our promise.—JAMES DEAS. Never heard before that the Star in the East was thought by any one to be the variable Mira. That star has been said to be a comet, a conjunction of planets, and a temporary meteor. Astronomy has nothing to say to any of these ideas.—S. DE MERVIGNE. The passages you quote show that where the conditions remain the same, races change little or not at all. What is there in the theory of evolution to suggest that they should change a great deal?—F. F. Your suggestion about the use of Jacob's ladder at fires seems excellent, but space should be found for it elsewhere. It does not belong so much to our subjects, as to those treated of in weekly newspapers or mechanical papers.—VIGNOLES. Many thanks; but your square has already been given.—ROBERTUS. We do not know the name of the secretary of the Society for Promoting the Abolition of Capital Punishment.—E. M. Not knowing the tables that you refer to, cannot tell precisely how the discrepancies arise. The date of your tables is so remote you could scarcely expect calculations made by them to correspond with the recent ones of the Nautical Almanack. We shall give such a map as you mention, relating to the transit of Venus. You must not expect to find in the "Nautical Almanack" all that is necessary to calculate phenomena. I had to work many hours and many days to get the results on which I based my investigations of the transits of 1874 and 1882.—J. X. DEVINE. Of course you are right. Our mistake was a natural one; but it was unquestionably a mistake. We have altered it, noting the error.—MEMBER OF THE ARCHÆOLOGICAL SOCIETY. Thanks; but it would be an obligation if you could briefly sketch for readers of KNOWLEDGE the essential ideas of Naville's book. The subject is a curious one, a little out of my own personal line of studies, but full of interest, and I am sure readers would be interested.

MR. W. H. H. HUDSON, late Fellow and Lecturer of St. John's College, Cambridge (Third Wrangler in 1861), succeeds Mr. Drew as Professor of Mathematics at King's College, London.

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Notes on Art and Science.

FERMENTATION IN BEER.—"In Re" is evidently unaware of the lengthy explanation which his question involves. In the first place, different brewers have different ways of starting fermentations. The Germans, and a few English, mix their yeast with a little of the "wort" first, and then add the rest. The Japanese use rice must for starting. Then the "pitching" variations during process of specific gravities and temperature must be considered, also germs generated in fermenting—healthy and unhealthy—the preparation of the wort and yeast; in fact, I should advise "In Re" to get a book on brewing and read it through, for it will *all* be interesting, and will lead him up to his subject. If Dr. Graham, Professor at University College, London, has published his lectures delivered on the subject, I can only say that "In Re" will have a book written by one who has mastered thoroughly the whole subject of brewing. Mr. T. A. Tooley, B.Sc., F.C.S., editor *Brewers' Guardian*, has his articles on Brewing and Distilling, written for the "British Manufacturing Industries," published. His name needs no comment.—F.C.S.

THE EXPANSION OF WATER BY HEAT.—Herr P. Volkmann has in the *Annalen für Physik und Chemie* compiled the results of Hagen, Matthiessen, Pierre, Kopp, and Jolly, on the expansion of water, and has obtained the following mean results for the volume and density of water at various temperatures:—

Temp.	Volume.	Density.	Temp.	Volume.
0 degr. C ...	1·000122	0·999878	15 degr. C.....	1·000847
1 ,, ...	1·000067	0·999933	20 ,, ...	1·001731
2 ,, ...	1·000028	0·999972	25 ,, ...	1·002868
3 ,, ...	1·000007	0·999993	30 ,, ...	1·004250
4 ,, ...	1·000000	1·000000	40 ,, ...	1·007700
5 ,, ...	1·000008	0·999992	50 ,, ...	1·011970
6 ,, ...	1·000031	0·999969	60 ,, ...	1·016940
7 ,, ...	1·000067	0·999933	70 ,, ...	1·022610
8 ,, ...	1·000118	0·999882	80 ,, ...	1·028910
9 ,, ...	1·000181	0·999819	90 ,, ...	1·035740
10 ,, ...	1·000261	0·999739	100 ,, ...	1·043230

—*Scientific American.*

SUNDAY LECTURE SOCIETY.—In the course of his lecture on "The Heart and its Work," on a recent Sunday, at St. George's Hall, Langham-place, Dr. Andrew Wilson gave some interesting details regarding the action of the central engine of the circulation. The heart, he showed, was merely "a hollow muscle," and the force which drives blood through our bodies is therefore similar to that wherewith we move our fingers in writing, or our legs in walking. Dr. Wilson showed also that the work of a man's heart in 24 hours amounted to 124 ft. tons; *i.e.*, if the heart's force for 24 hours were gathered into one stroke or lift, it would suffice to lift 124 tons 1 ft. high. The heart is ruled by at least three sets of nerves. In its own substance, there are masses of nerve-matter, or *ganglia*, which carry on the normal work of the organ. Then, secondly, there exists an important *vagus* nerve, which checks or slows the heart's action, as under the influence of fear or other mental emotion; and there is, thirdly, a *sympathetic nerve*, which causes the heart's action to increase in rapidity. The varying emotions of the mind thus influence the heart for good or for evil by disturbing its normal action.

NO ORGANIC MATTER IN METEORS.—A Louisville (Ky.) paper reports an interview with Prof. J. Lawrence Smith, of that city, in the course of which Mr. Smith gave reasons for discrediting the discovery of organic substances in meteors, as claimed by Prof. Hahn, of Berlin. Mr. Smith said:—"Although I have probably examined more microscopic plates of fragments of meteorites than any other person, still I have never discovered anything like organic remains in any of them. Besides, the well-known chemical composition of these bodies is averse to the existence of any such remains as spoken of by Prof. Hahn. Were these remains present, we should discern carbonate of lime in their interior. The two or three that have any carbonate of lime were discovered and analysed by myself, and in these cases the carbonate of lime was an accidental constituent of incrustation deposited on the surface after their fall. In the microscopic examination of these polished plates of meteorites, the two predominating minerals, enstatite and bronzite, will, by their fissures and forms, sometimes remind one of vegetable and other organic forms, but the merest tyro of an observer will trace here nothing but a rare resemblance. And furthermore, the nature of these minerals precludes the possibility of organic remains even in terrestrial minerals of similar kind. Not knowing of any eminent German geologist named Prof. Hahn, I thought it but reasonable and logical that I should inquire something about him from my friend Prof. Hawes, now in the employ of

the Smithsonian Institution, and the best lithological microscopist in this country, and who recently returned to this country after ten years' study with Prof. Rosenbaum and others into the microscopic character of rock. In answer to my inquiries, Prof. Hawes wrote me this letter:—"I read that paper of Prof. Hahn's. He is a kind of half-insane man, whose imagination has run wild with him. These forms which he so accurately describes and figures have long been known to exist in meteorites, and have been frequently described by mineralogists and microscopists. They are mainly composed of enstatite or bronzite in radial forms, and fractured in such a peculiar manner as to give them the appearance of structure. Some of the American meteorites which I have examined show these forms in great beauty, but Prof. Hahn is the only man who has seen anything organic in them, and his paper has excited nothing but ridicule. It reminds one of the long and laborious research of a German professor who found a whole flora and fauna which he named with double Latin names, and which he found in his microscopic examination of basalt. It is very clear to my mind," continued the Professor, "that these cranky observations, viewed with the spectacles of the imagination of Prof. Hahn, have obtained more publicity than they merit."—*Scientific American*.

Our Mathematical Column.

MATHEMATICAL QUERIES.

[15]—ARITHMETICAL PROBLEM.—A and B have to build a wall one hundred yards long; A is to have five shillings a yard more than B. When the wall is finished A and B each receive £50. How many yards did each do, and at what rate per yard?—HAMILTON STUART.

[This leads to an indeterminate equation. Thus, if x be number of yards built by A, y the number of shillings received per yard by B, we have $x(y+5)+y(100-x)=2{,}000$; or $x+20y=400$.—ED.]

[16]—In an old volume of problems, I came across the following:—A messenger, M, starts from A to B (a distance a miles), at a rate of v miles per hour, but before he arrives at B, a shower of rain commences at A, and at all places occupying a certain distance, z, towards, but not reaching beyond, B, and moves at the rate of u miles an hour towards A. If M be caught in the shower, he will have to wait until it is over. He is also to receive for his errand a number of shillings inversely proportional to the time occupied in it, at the rate of n shillings for one hour. Supposing the distance, z, to be unknown, as also the time at wkich the shower commenced, but all events to be equally probable, show that the value of M's expectations, in shillings, is—

$$\frac{nv}{a}\left[\frac{1}{2}-\frac{u}{v}+\frac{u(u+v)}{v_2}\log.\frac{u+v}{u}\right]$$

Can you or one of your readers kindly solve this problem for me without using the differential calculus?—NO ANALYST. [We will leave this problem for a week. "No Analyst" has set us a hard one.—ED.]

[17]—TORTOISE PROBLEM.—A hare and tortoise have a race, the hare gives the tortoise 100 yards start and runs ten times as fast; they start together, but while the hare runs the 100 yards, the tortoise has crawled ten yards; while the hare is running the ten yards the tortoise crawls one yard; while the hare runs that one yard the tortoise has advanced $\frac{1}{10}$ of a yard; and so on, *ad infinitum*, the hare, mathematically, never overtaking the tortoise.—TORTOISE. [The paradox was known to the Greeks as the problem of Achilles and the Tortoise. The explanation is that the distance run by either animal is divided into an infinite number of parts, forming a geometrical series, having a finite sum. Thus the hare runs 100 yards $+10+1+$ &c. $=100+(1-\frac{1}{10})=100\left(\frac{10}{9}\right)=111\frac{1}{9}$ yards. The tortoise crawls $11\frac{1}{9}$ yards. Dividing up a finite distance into an infinite number of parts does not make it infinite.—ED.]

[18]—ARITHMETICAL QUESTION.—A man having a cask containing 300 gallons of wine takes out one gallon per day, putting in a gallon of water to refill the cask each day. After how many days will the mixture in the cask be half water and half wine.—YOUNG BEGINNER. —[Y. B. asks whether this problem can be solved without the aid of logarithms. Not readily. At end of one day the mixture is $\frac{299}{300}$ ths wine, at end of second it is $\frac{299}{300}$ ths of $\frac{299}{300}$ ths wine or $\left(\frac{299}{300}\right)^2$ wine, and so on. If in x days the mixture in the cask is half water and half wine, we have $\left(\frac{299}{300}\right)=\frac{1}{2}$; or taking logarithms of reciprocals of each side, we have $x(\log.\ 300-\log.\ 299)=\log.\ 2$, or $14501x=3010300$, $x=\frac{3010300}{14501}=207{\cdot}59$; or up to the 208th day the mixture would contain more wine than water, after the 208th day it would contain more water than wine.]

A. M. R. points out a mistake on p. 191, where we put

$$1=(\sqrt{1+x}+x)\ (\sqrt{1+x}-x)$$

Of course, this relation does not hold; and the solution fails. We had found 0 to be the only root which could be obtained without a solution for cubic or biquadratic, and as the solution, after above mistake, led to root 0, we did not (as we should have done), run through the steps. The equation obtained just before the mistake occurs is

$$1=\sqrt{1-x}\ (\sqrt{1+x}+x)$$

this will be found to give $x=0$ and the biquadratic

$$x^4-4x^3+4x^2+4x+4=0.$$

"T. R." considers that the proposition $DM^2=AM\,.\,MB$, first figure, p. 214 (ADB a right-angled triangle, and DM perp. to AB), may be established without the use of books of Euclid beyond I. and II. Thus (we rather abridge his proof):

$$AM^2+MB^2+2AM.MB=AB^2=AD^2+DB^2=AM^2+MB^2+2MD^2$$
$$\therefore\ AM.MB=MD^2$$

We did not say otherwise. We said it neither is in those books, nor can be given as a *corollary* on any propositions they contain. Several of the propositions in Book III. can be as easily proved from Book I., but they are thrown, for convenience, into a separate book.

[19]—SUM OF SQUARES.—I do not know if the following simple method of getting at the formula for $1^2+2^2+3^2+4^2\ .\ .\ .\ .\ +n^2$, is new or not, but I believe it is not the usual one, and so send it on the chance of your thinking it worth publishing:—

	1	2	3	4	5	.	.	.	n	A
	1	2	3	4	5	.	.	.	n	
	1	2	3	4	5	.	.	.	n	
C	1	2	3	4	5	.	.	.	n	B
	1	2	3	4	5	.	.	.	n	
	.	.	.	.	.	.	.	.	.	

Here the horizontal line cuts off the series $\frac{n}{2}(n+1)$, and the zig-zag line divides the figures forming the squares from the unnecessary ones below. The sum of the squares equals $A+B$.

Now, $B=2C$, as is readily seen from the above diagram. [Every vertical column in B sums up to twice the sum of a horizontal row in C.—ED.] And

$$B+C=(n-1)\times\frac{n}{2}(n+1)$$
$$\therefore\ B=\frac{2}{3}(n-1)\times\frac{n}{2}(n+1)$$
$$=\frac{n}{3}(n-1)\ (n+1)$$
$$\therefore\ A+B=\left(\frac{n}{2}+\frac{n}{3}(n-1)\right)(n+1)$$
$$=\frac{n(n+1)\ (2n+1)}{6}$$

FLORENCE E. BOYCE.

[9]—Several correspondents consider this problem incorrect, incomplete, or absurd. Others give incorrect solutions. It is, however, quite correctly stated. It is correctly solved thus, or to the same effect, by T. R., Charles Hammond, D. Kelley, Jas. Frobisham, K. Carlson, and others. We give Mr. Hammond's solution as one of the neatest in form:—

"Since the grass grows uniformly, its growth may be kept down by a certain number, independent of time, but varying directly as the area of the field.

"The remaining horses are required to consume the original stock of grass. Their number varies directly as the area, and inversely as the time.

"From this it is evident that in the first case 6 horses take 16 weeks to consume the original stock of 10 acres, and 6 keep down the growth.

"Now, by proportion, the number of horses taken to consume the original stock in a 40-acre field in 6 weeks is 64, and the number required to keep down the growth is 24.

∴ Answer, 88 horses.

N.B. For the general case of this, see Newton's "Arithmetica Universalis," page 90, 2nd edition, London, 1722.

Our Whist Column.

BY "FIVE OF CLUBS."

THE LEAD (*Continued*).

ADDENDUM TO PLAIN SUIT LEADS.

THE leads considered in the last two numbers were supposed to be either original leads from long or strong suits, or forced leads with no knowledge of your partner's strength in the suit led. Note that in every case of a forced lead from a short suit, where you have reason to believe that your partner has strength in the suit, the highest is played, so that from Ace two others you lead Ace in this case, following with the next highest. Similarly, from King two others you play King, then next highest; from Queen two others, Queen, then next highest; from Knave two others, Knave, then next highest. This last is the constant lead from Knave two others (as from ten two others, you play ten, then next highest): when your partner has not indicated strength, there is some use in leading lowest from Ace two others, King two others, or Queen two others; but manifestly keeping back the Knave or ten (with two others) can be of no use to you, while playing it may help your partner.

THE LEAD IN TRUMPS.

We do not propose to consider here the important question (important in whist-play, at any rate) when to lead trumps, but what card is to be led when it has been decided to open the trump suit.

The lead in trumps differs in one important respect from the lead in plain suits,—there is no fear that a good card will be lost if kept back, by being trumped. We can, therefore, play with safety a waiting game; indeed, it is often advantageous to do so, because so much often depends on winning the last round in trumps.* Another difference between trump cards and others arises from the circumstance that you need not so carefully indicate your strength by playing an obviously winning card; for there is no possibility of your partner hurting you by trumping a card which he may mistake for a losing one. The chief variations of the trump lead from a lead in plain suits depend on these considerations—principally on the former.

Thus, in trumps, from Ace, King, and not more than five others, the smallest should be played; this ensures the numerical command in trumps, if you have five others, and is the best way towards obtaining it if you have less than five. Besides, by this course you give your partner a good chance of winning the first trick. Of course, if you have six small ones besides Ace and King, you have the numerical command, even if all the remaining trumps are in one hand; you therefore play King, then Ace.

Again, from Ace, King, Queen alone in plain suits, you lead King, then Queen. In trumps, having no fear that Queen will be lost, you lead Queen first, then King. So with Ace, King, Queen, Knave, you lead King first in plain suits; in trumps you lead the lowest of the sequence, the Knave.

Again, in plain suits from Ace and four or more others, not including King, you lead Ace, because of the risk that in the second round it might be trumped. Having no such fear in the trump suit, you lead the smallest, unless you have more than six small ones, in which case, being sure of the numerical command, you lead Ace.

In trumps from King, Queen, and two or more small ones, the lowest is led, instead of the King, as in plain suits; but with more than five small ones, begin with King.

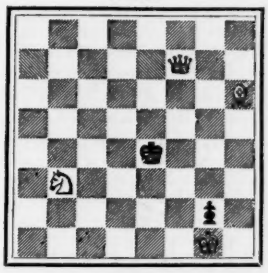
Some trump leads differ from plain suit leads and ordinary trump leads, on account of the trump card being of a particular value. Thus, from Ace, Knave, ten and nine, the nine would be led in plain suits: so, also, nine would be led in trumps, unless Queen is turned up on the left, when Ace should be played. Again, from King, Knave, two or more small ones, the lowest is led in plain suits, and ordinarily in trumps; but if ten is turned up to the right, the Knave is led. In trumps, from Knave, ten, eight, with one or more small ones, lead the lowest, as in plain suits, unless nine is turned up on your right, when lead Knave.

* We do not mean simply having the last winning trump, for this may be left in and the opponents' strong suits yet played out to the bitter end; but winning the last round of trumps, so as to be able to bring in a strong suit either of your own or your partner's.

Our Chess Column.

PROBLEM No. 11 (From *American Chess Nuts*).
By E. B. Cook.

BLACK.



WHITE.

White to play and mate in three moves.

TWO KNIGHTS' DEFENCE (*Continued*).

ANOTHER interesting form of this opening is where Black, on his fourth move, plays Kt. takes K.P. In most cases strong players will adopt this move with good effect against a weaker opponent. As it is chiefly our wish to instruct the student, we shall show him in full all the variations resulting disadvantageously, so that he might avoid them, at the same time also giving him the good replies for adoption.

1. P. to K.4.	2. Kt. to K.B.3.	3. B. to B.4.	
P. to K.4.	Kt. to Q.B.3.	Kt. to K.B.3.	
4. Kt. to Kt.5.			
Kt. takes K.P.			

White has three modes of continuing, viz., Kt. takes K.B.P. or Kt. takes Kt., or B. takes P. ch. (best). Of these only the last move can be recommended, for if

5. Kt. takes K.B.P.			
Q. to R.5.			
6. Q. to K.2.	or 6. Castles.	or 6. P. to K.Kt. 3.	
Kt. to Q.5.	B. to B.4.	Kt. takes Kt.P.	
7. P. to K.Kt.3.	7. Kt. takes R.	7. B.P. takes Kt.	
Kt. takes Q.,	Kt. takes B.P.	Q. to K.5.ch.	
8. P. takes Q.	8. B. to B.7.ch.	8. Q. to K.2.	
Kt. to Q.5.	K. to K.2.	Q. takes R.ch.	
9. P. to Q.3.	9. R takes Kt.ch.		
Kt. to Q.3.	Q. takes R.ch.		
10. Kt. takes Kt.ch.	10. K to R.sq.		
B. takes Kt.	B. to Q. 3.		

In all these cases Black obtains a decisive superiority. 5. Q. to R.5 wins in any case, as the attack obtained thereby is very strong.

If 5. Kt. takes Kt.	6. B. takes P.	7. Q. Kt. to B.3.
P. to Q.4.	Q. takes B.	Q. to Q.sq.

and the game is even

If 5. B. takes P.ch. (best)

K. to K.2.			
6. P. to Q.3.	or 6. Kt. takes Kt.	or *A*.	
Kt. to K.B.3.	K. takes B.		
7. B. to Q.Kt.3.	7. Q. to B.3.ch.	or 7. Q.Kt. to B.3.	
P. to Q.4.	K. to K.sq.	P. to K.Kt.3.	
8. P. to K.B.4.	8. P. to Q.4.	8. Castles.	
B. to K.Kt.5.	P. to Q.4.	P. to Q.4.	
9. Q. to Q.2.	9. Kt. to Kt.5.	9. Kt. to Kt.3.	
K. to Q.2.	Q. to B.3.	B. to K.3.	
10. Castles.	10. Q. takes P.	10. P. to Q.3.	
	Kt. takes P.	B. to Q.B.4.	

It will be recognised that Black incurs the disadvantage of playing his K. to K.2., on account of the strong centre which he afterwards threatens to obtain, which, as second player, he could not get in the ordinary course. In all the three variations given above, White has no advantage, but Black has a great many chances of getting the better game. In the first variation the game is about even, although Black will have to take some care. We can best show the very attacking nature of the opening in spite of the unfavourable position of Black's King (which, as we have before said, is compensated by his

strong centre) by giving the following additional variations, namely, if in the first variation, instead of 10 Castles, White play the tempting move 10. Kt. to B.7. Black gets the advantage as follows:—

10. Kt. to B.7. / Q. to K.sq. 11. Kt. takes R. / P. takes P.dis.ch. 12. K. to B.sq. / Kt. to Q.5. 13. Kt. to B.3. / B. to Q.B.4. , and Black has the better game, as he has an exceedingly strong attack. He chiefly threatens Kt. to B.6, which White cannot retake with the Pawn, on account of B. to R.6.ch. The Queen has no place to go to if attacked by the Kt., as Q. to K.8.ch. is thereby threatened. White would lose his Queen if, in reply to Kt. to B.6. he should play Q. to Q.sq.,—by Kt. takes P.ch.

Again, if in the first variation White does not play 8. P. to K.B.4. he will speedily be strongly attacked by Black. If, for instance, 8. Castles. / P. to K.R.3. 9. Kt. to K.B.3. / K to B.2. , with a good game, as Black has two good plans to pursue, firstly B. to Q.3., R. to K.sq., and K. to Kt.sq.; or, secondly, he might play B. to Q.3, B. to K.3., Q. to Q.2., and Q.R. to K.Kt.sq., so as to proceed with an attack on the King's side by P. to K.Kt.4.

Of course, statements of these kind are only general, and meant to show the nature of the position, but real play has, of course, to be modified, even according to weak replies of an opponent. But, whatever White should do, Black should obtain the better game, owing to his good position.

(*A*) 1. P. to K.4. / P. to K.4. 2. Kt. to K.B.3. / Kt. to Q.B.3. 3. B. to B.4. / Kt. to K.B.3. 4. Kt. to Kt.5. / Kt. takes K.P. 5. B. takes P.ch. / K. to K.2. 6. P. to Q.4.

This is the very best reply, we may say the only move which, amongst the numerous possible moves, gives White the advantage, which fact shows the dangerous nature of this opening. 6. P. to Q.4. breaks up Black's centre, and thereby obtains for White the better game.

In reply to 6. P. to Q.4. Black can play

6.	or 6.	or 6.
P. to Q.R.3.	P. to Q.4.	Q.Kt. takes P.
7. Kt. takes Kt.	7. P. takes P.	7. P. to Q.B.3.
K. takes B.	Q.Kt. takes P.	Kt. to Q.B.3.
8. P. to Q.5.	8. Q. takes P (best)	8. B. to Q.5.
Kt. to K.2.	Q. takes Q.	Kt. to K.B.3.
9. Q. to R.5.ch.	9. B. takes Q.	9. Kt. to B.7.
P. to Kt.3.	Kt. to K.B.3.	Q. to K.sq.
10. Q. takes K.P.	10. Kt. to Q.B.3.	10. Kt. takes R.
B. to Kt.2.	P. to B.3.	Kt. takes B.
11. Q. to B.4.ch.	11. B. to Kt.3.	11. Q. takes Kt.
K. to Kt.sq.	B. to B.4.	P. to K.R.3.
12. Q.Kt. to B.3.	12. B. to K.3.	12. Q. to K.4.
winning.	and White has a good game.	winning.

Besides these moves Black can also play

6.	6.	6.
P. to Q.3.	P. takes P.	Kt. to K.B.3.
7. B. to Q.5.	7. Q. to K.2.	7. P. takes P.
Kt. to B.3.	P. to Q.4.	Kt. takes P.
8. B. takes Kt.	8. B. to R.5.	8. Q. to K.2.
P. takes B.	etc.	etc.
9. P. takes P.		
etc.		

If 6. Kt. to Q.3. White wins the Queen by 7. Kt. to K.6. Therefore, whatever Black may do, White, by 6. P. to Q.4., will invariably get the better game, which move, therefore, is the proper defence against this strong counter-attack.*

* To Mr. Gossip is due the credit of advocating this strong move and the ensuing variations.

SALFORD.—You have not transgressed the rules of chess, but your solution of Problem 6 is unsound. After Q. to Q.3.ch., B. takes Q., P. takes B., why should not Black take B. with P. in his turn? How would White mate then on the move?

JAMES MACALPINE.—If (1) Kt. to Kt.5. in Problem 6, 1. P. to Kt.4., 2. P. takes K.P., Black can play Q. to Q.'s 5th ch., and there is no mate.

CRYPTOTYPE.—If Q. takes K.B.P., in Problem 6, Black moves Q. to Q.R. square; then, if Q. takes Q., P. to Q.B.3., and there is no mate on the move.

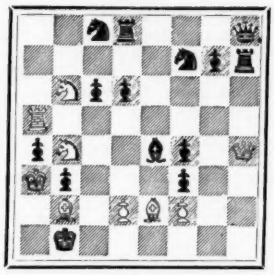
H. FITZ HART.—Your letter forwarded to Hades, "Mephisto," being the author of the analysis in question. You should have addressed Chess Editor.

The *Chess Player's Chronicle* calls our attention to the following beautiful problem by Mr. Grimshaw, in which the idea underlying the second solution of his problem, No. 6, p. 190, is embodied.

PROBLEM, No. 12.

By W. Grimshaw.

BLACK.



WHITE.

White to play and mate in four moves.

Unfortunately there are two solutions, one beginning Q. takes P., the other (the author's) beginning Q. takes R. (at R.'s 2.). The *Chronicle* believes that the solution given in the *Illustrated London News* was not the author's; how, then, was it that no reference was made in the *Illustrated* to any other solution? The *Chronicle* appends a note of interrogation to our remark that if, in Problem 6, Queen goes at once to K.'s 6th, mate is only threatened in one way. How is mate threatened, except by B. takes P.? After 1. Kt. to K.B.5., 1. B. takes Kt. 2. Q. to K.'s 6th, mate is threatened also by Q. takes B.,—that is, as we said, in two ways.

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